

# VU Research Portal

## Reflections on scientific misconduct in management

Honig, Benson; Lampel, Joseph; Baum, Joel A.C.; Glynn, Mary A.N.N.; Jing, Runtian; Lounsbury, Michael; Schüssler, Elke; Sirmon, David G.; Tsui, Anne S.; Walsh, James P.; Van Witteloostuijn, Arjen

### **published in**

Academy of Management Perspectives  
2018

### **DOI (link to publisher)**

[10.5465/amp.2015.0167](https://doi.org/10.5465/amp.2015.0167)

### **document version**

Publisher's PDF, also known as Version of record

### **document license**

Article 25fa Dutch Copyright Act

[Link to publication in VU Research Portal](#)

### **citation for published version (APA)**

Honig, B., Lampel, J., Baum, J. A. C., Glynn, M. A. N. N., Jing, R., Lounsbury, M., Schüssler, E., Sirmon, D. G., Tsui, A. S., Walsh, J. P., & Van Witteloostuijn, A. (2018). Reflections on scientific misconduct in management: Unfortunate incidents or a normative crisis? *Academy of Management Perspectives*, 32(4), 412-442.  
<https://doi.org/10.5465/amp.2015.0167>

### **General rights**

Copyright and moral rights for the publications made accessible in the public portal are retained by the authors and/or other copyright owners and it is a condition of accessing publications that users recognise and abide by the legal requirements associated with these rights.

- Users may download and print one copy of any publication from the public portal for the purpose of private study or research.
- You may not further distribute the material or use it for any profit-making activity or commercial gain
- You may freely distribute the URL identifying the publication in the public portal ?

### **Take down policy**

If you believe that this document breaches copyright please contact us providing details, and we will remove access to the work immediately and investigate your claim.

### **E-mail address:**

[vuresearchportal.ub@vu.nl](mailto:vuresearchportal.ub@vu.nl)

See discussions, stats, and author profiles for this publication at: <https://www.researchgate.net/publication/320081188>

# Reflections on Scientific Misconduct in Management: Unfortunate Incidents or a Normative Crisis?

Article in *Academy of Management Executive* · September 2017

DOI: 10.5465/amp.2015.0167

CITATIONS

3

READS

273

11 authors, including:



**Benson Honig**

McMaster University

127 PUBLICATIONS 6,611 CITATIONS

[SEE PROFILE](#)



**Joseph Lampel**

The University of Manchester

113 PUBLICATIONS 4,853 CITATIONS

[SEE PROFILE](#)



**Joel A. C. Baum**

University of Toronto

160 PUBLICATIONS 12,701 CITATIONS

[SEE PROFILE](#)



**Michael Lounsbury**

University of Alberta

138 PUBLICATIONS 11,176 CITATIONS

[SEE PROFILE](#)

Some of the authors of this publication are also working on these related projects:



Research Practices in Management [View project](#)



Kolvereid (19996) Scale Validation [View project](#)

# A R T I C L E S

---

## **REFLECTIONS ON SCIENTIFIC MISCONDUCT IN MANAGEMENT: UNFORTUNATE INCIDENTS OR A NORMATIVE CRISIS?**

### **EDITORS**

**BENSON HONIG**  
McMaster University

**JOSEPH LAMPEL**  
University of Manchester

### **CONTRIBUTORS**

**JOEL A. C. BAUM**  
University of Toronto

**MARY ANN GLYNN**  
Boston College

**RUNTIAN JING**  
Shanghai Jiao Tong University

**MICHAEL LOUNSBURY**  
University of Alberta

**ELKE SCHÜBLER**  
Johannes Kepler University, Linz

**DAVID G. SIRMON**  
University of Washington

**ANNE S. TSUI**  
University of Notre Dame

**JAMES P. WALSH**  
University of Michigan

**ARJEN VAN WITTELOOSTUIJN**  
VU Amsterdam

**Taking as our starting point Merton's (1942/1973) defense of science facing pressures from totalitarian regimes, we argue that today's challenge to the integrity of management scholarship does not come primarily from external demands for ideological conformity, but from escalating competition for publication space in leading journals that is changing the internal dynamics of our community. We invited nine scholars from different countries and with different backgrounds and career trajectories to provide their brief views of this argument. Following an introduction that summarizes the argument, we present their different reactions by dividing and introducing the work into those who took a broad field-level perspective, those with a more macro view, and those who suggested possible remedies to our dilemmas. In conclusion, we note that questionable research practices, retractions, and highly publicized cases of academic misconduct may irreparably damage the legitimacy of our scholarship unless the management research community airs these issues and takes steps to address this challenge.**

"Science would be ruined if (like sports) it were to put competition above everything else."

— Benoit Mandelbrot (cited in Gleick, 1987)

Unease about the state of contemporary scientific research has itself become a research topic of growing interest (De Vries, Anderson, & Martinson, 2006; O'Boyle, Banks, & Gonzalez-Mulé, 2014). The symptoms of a malaise are certainly hard to miss; something may indeed be rotten in the state of Denmark: Retractions are on the rise, instances of gross misconduct are frequently in the headlines, authors complain about coercive citations, predatory journals shamelessly solicit contributions, and gaming the publication process is increasingly viewed as the smart road to success (Fanelli, 2010b; Martin, 2013).

A common response to these symptoms is to tighten the rules and call attention to questionable research practices (John, Loewenstein, & Prelec, 2012). But we cannot understand these symptoms, or design remedies, without noting that our scholarly universe faces unparalleled disruption both technologically and socially (Martin, 2013). This disruption brings to the surface the perennial tension in scholarly communities between competition for reputation and resources and the norms that protect our research integrity. Scholarly communities have very high (and perhaps unrealistically elevated) expectations that individual researchers will adhere to these norms. But the reality, as sociologists of science amply demonstrate, is that the academic enterprise is a social process, navigated by individuals and institutions with all the fallibilities of any human endeavor, and that as management scholars, we are no more exempt from the limitations and weaknesses of judgment than those inhabiting other public arenas, including politics, sports, and jurisprudence.

The demands of contemporary scholarship place increasing pressures on management scholars to publish in peer-reviewed journals with high impact factors calculated with mathematical precision

(Miller, Taylor, & Bedeian, 2011). While new technologies affect how we communicate and confer, they also reinforce change in the geopolitics of science. The dominance of Western countries, and their preeminence in scientific research, can no longer be taken for granted. The remarkable rise of China as a scientific and research power is a case in point. Chinese scholars recently published 38 papers in the *Academy of Management Journal*, representing over 10% of authorship (Yang, 2016).

The emergence of China as a strong scientific power is due in part to a strong economic base capable of supporting research, but it is also due to government policies that offer monetary incentives for authors who publish in internationally recognized journals. This in turn has given rise to companies that secure fraudulent reviews and "paper brokers" who charge as much as \$5,500 per authorship (Hvistendahl, 2015). For authors who cannot publish in reputable journals, there is the temptation of fake journals and a black market for invented research with an estimated turnover of \$150 million per year (Economist, 2013). At first sight, this kind of rampant commercialism may shock researchers from elsewhere, in particular researchers from countries where modern science dates back several centuries. However, before we dismiss this as the product of perverse incentives that are unique to China, we must remember that bonus payments for publishing in top journals are fairly widespread in the West, not to mention the six-figure salaries, light teaching loads, and tenured lifetime employment enjoyed by researchers at many top universities. We take it for granted that these incentives are important for giving researchers the resources and personal space required for high-quality research, but we are often reluctant to acknowledge that they foster competition that may lead to behavior that possibly undermines the legitimacy of our profession, both internally and externally.

We resist this conclusion because we have a strong belief in the integrity of the research system, with its checks and balances. And arguably, we also resist this conclusion because of a strong belief that our colleagues' loyalty to the values of science will insulate them from the temptation of finding shortcuts that compromise their research findings.

The values of science flow from the way science is done, from the norms that guide and constrain the research process. This underpins the legitimacy of science as an institution and as a process. It is commonplace to say that all institutions depend on a measure of legitimacy, but arguably scientific institutions depend on legitimacy more than most. When external audiences query the legitimacy of science—the “why science?” so to speak—scientists point to the research process; they point to how science is done. The integrity of how research is conducted is crucial for legitimizing science externally. Laypersons and the public at large assume that scientific training and socialization inculcate researchers with methodological and moral codes that preclude shortcuts that compromise ethics or quality. Nor is the belief in integrity displayed solely for external consumption; it is also important to internal legitimacy: The more convinced researchers are that their colleagues uphold high standards, the more effective they can be as researchers, as teachers of the next generation of researchers, and as advocates for science in the wider society.

What we take for granted today was not always so. In the history of science, powerful actors such as the church or the state have often maintained that science should be subordinate to the dictates of its paymasters. Galileo was famously held under house arrest by the Inquisition, while Charles Darwin withheld his initial findings for 20 years out of fear of offending contemporary religious views. For the ecclesiastical authorities, science, if necessary, had to modify the way it conducted research when in conflict with a prevailing ideology. The conflict receded during the 19th century but escalated once again in the first part of the 20th century, at a time when both fascism and Soviet communism overtly coerced and rewarded scientists according to their willingness to allow ideological strictures to modify the research process. Scientists sometimes resisted but all too often went along, either passively, by deleting findings that undermined the official position of the party in power (such as burying inconvenient demographic or statistical data), or actively, by fashioning the research process to deliver results that reinforced totalitarian ideologies (such as eugenics).

Although this surrender is not so remote in time, it seems remote in memory: Fascism was defeated, and

Soviet communism eventually collapsed. The value of science to society has been so widely established and taken for granted that challenge to its legitimacy seemed unimaginable. What also seemed unimaginable is the possibility that the threat to the legitimacy of science would come not from external actors who subvert the independence and autonomy of the research process but from concern that the integrity of the research process is being subverted by the scientists themselves. These doubts are sometimes expressed in relation to specific incidents of scientific misconduct, as well as in decline narratives that argue that the integrity of the research process is fracturing under the pressure of intense competition for scarce publication space.

A cursory look at the website Retraction Watch,<sup>1</sup> which aggregates retractions across a large number of journals worldwide, adds to this sense of unease. Publicly, the response has been uncertain; we are struggling with how we, as a community of researchers, can address these doubts. Privately, this has become a subject of frequent spontaneous discussions in the field of management, as well as in other fields. It is this spontaneity, arising from firsthand experience, that we wished to capture when we embarked on this AMP article. Rather than ask people to contribute to this debate in the typical academic fashion—for example, via a call for a special issue—we decided to be deliberately provocative in the hope that our provocation would elicit strong personal views. To provoke we drafted and sent a letter (see appendix) to select scholars drawing a parallel between the doubts that are expressed about research integrity today and the attack on scientific autonomy in the 1930s and 1940s. We used Robert Merton's famous 1942 essay “The Normative Structure of Science”<sup>2</sup> (Merton, 1942/1973) to set up the comparison. Of course, the autonomy of scientific research today is not under attack from totalitarian states (at least, not in most places) but rather is a challenge from within, caused by intense competition that fosters the kind of entrepreneurialism that leads researchers to discard the normative guidelines that are central to the legitimacy of science.

Drafting an invitation letter for potential contributors was the first step in this process. The next step was to ensure a diversity of views by calling on scholars with a range of backgrounds and positions. We therefore invited both researchers who are just starting their careers and senior scholars with established reputations. We made special efforts to invite scholars from Europe and Asia as

<sup>1</sup> <http://retractionwatch.com>

<sup>2</sup> [https://www.collier.sts.vt.edu/5424/pdfs/merton\\_1973.pdf](https://www.collier.sts.vt.edu/5424/pdfs/merton_1973.pdf)

well as North America. We included three past presidents of the Academy of Management. It is fair to say that the response from these authors exceeded our expectations, but we will let you, the reader, make up your own mind.

While we had no particular anticipation regarding what responses (if any) we would receive from this project, we were quite surprised by both the enthusiasm and the rigor of those scholars who responded. (For the record, 10 out of 13 contacted scholars agreed to participate in this project, and nine continued through the entire process.) The selected essayists went through numerous drafts, often obtaining friendly reviews on their efforts, and updating us with their most thoughtful responses. All took their essays very seriously.

Although there were many overlapping thoughts and concerns expressed by these scholars, we observed patterns that allowed us to examine the essays from two different analytical perspectives, providing the reader with a framework with which to evaluate the project. The first division we noted was between those that took a broader, field-level perspective and those that focused primarily on specific problems and issues. Essentially, we might consider this a division between micro and macro approaches, although this was not necessarily the stated intention of the authors. The second division we noted concerned those scholars who provided concrete recommendations regarding what they thought might be done to help ameliorate some of the problems they discussed. Interestingly, not everyone made extensive recommendations, and opinions ran the gamut from suggesting specific micro processes to advocating wholesale institutional restructuring. We begin with a discussion of those responses that focused primarily on outlining the problem, before discussing those focusing on remedies.

### **SCIENTIFIC MISCONDUCT IN MANAGEMENT: ESSAYS TAKING A BROAD VIEW**

The first three essays we examine look at the field of management misconduct from a macro perspective. Rather than focusing on the instrumental or specific aspects of misconduct, they primarily discuss the overall set of norms and incentives that characterize the field, leading to some of the dilemmas we now face.

Michael Lounsbury begins by discussing the growth of bibliometrics, arguing that a continuous stream of citation reports has unduly influenced administrators, editors, scholars, and the academy itself. Deploring efforts to assess academic contributions through quantitative methods, Lounsbury worries that we have begun to value formulaic

presentations rather than novelty or innovation, incentivizing efforts to manipulate quantitative data in various ways. Interestingly, Lounsbury sees this institutional shift as part of a general social trend toward market logics, leading to formulaic conformity and a general narrowing of perspectives on worthiness. Lounsbury points out the decline of the humanities and social sciences in universities as one aspect of this trend and implores us to search for a more multidimensional solution toward a progressive agenda.

Anne Tsui takes a broad view as well, focusing on the value-laden implications of management scholarship. Historically, management research owes its existence to the rise in large business organizations and the issues this transformation raised for managers, employees, and society at large. But arguably, over time, many researchers have sought to emulate their colleagues in the traditional disciplines by advocating dispassionate value neutrality toward their work. Disabusing us of the notion of a value-free scientifically neutral ivory tower, Tsui argues that there is a tension between the value-free ideal and the social responsibility of universities. The distance between solving real-world problems and providing esoteric contributions to a very narrow audience of scholars leads to irresponsible outcomes. The result is a waste of resources that distances scholars from facing scientific failure. Tsui calls for management scholars to move from value-free instrumentalism toward responsible science and become a positive force for change.

Mary Ann Glynn takes a systemic view of the problems we face, arguing that the very systems we have designed for scientific conduct cultivate misconduct. Using the example of the space shuttle Challenger disaster, Glynn highlights the influence of social norms that come to rationalize deviance. Referencing a recent article by Barley, Glynn laments the change in scholarly expectations and how that has imperiled scholarly progress. Incrementalism, argues Glynn, will not suffice; what we need is wholesale systemic change.

### **Tragedy of the Commons: Quantifying Scientific Quality, by Mike Lounsbury**

While the growing use of journal impact factors (IFs) to assess the quality of journals as well as the worth of individual researchers and their scholarship has been widely critiqued (e.g., Baum, 2011; Brumback, 2008; Garfield, 2006), it is important to emphasize that the current fetishization of IFs has fostered variegated forms of scientific misconduct. Baum (2011, p. 464) argued that IF measurements are “ill-conceived, unreliable, and invalid” and that their growing use is

insidious, having “the potential to distort researcher and editorial behavior in ways that are highly detrimental to the field.” In fact, there is much evidence to suggest that this potential has been realized. And the nature of these problems is profound, given that the rise of IFs and associated misconduct is rooted in the broader embrace of quantitative assessments of quality across various societal domains (Espeland & Stevens, 1998).

The IF of an academic journal, calculated as the average number of citations to recent articles published in a journal, is conventionally used to indicate the importance of a journal relative to other journals in a discipline. IF measures, initially created in the 1960s, became increasingly attended to in the 1990s due to the aggressive marketing of Thomson Scientific (Baum, 2011). In the 1990s, the field of management seemed to have widely shared, and fairly ossified, beliefs about what the top journals in the field were, but growing attention to IFs, especially over the past decade, has begun to raise questions about previously taken-for-granted understandings. For instance, in 2014, the *Journal of Business Venturing* (JBV) boasted a three-year IF of 3.678. In the same year, the IF of *Administrative Science Quarterly* (ASQ) was 3.333. But such an example tends to raise more questions than it answers. Is it just as, or even more, prestigious to publish an entrepreneurship article in JBV as opposed to ASQ? Are JBV articles, on average, of higher quality than ASQ articles?

Baum (2011) showed that given the skewness in the distribution of citations to articles in management journals (where the majority of articles receive few citations), the IF as an average measure is a poor measure of quality. This kind of citation variance has been identified in other sciences (Bohannon, 2016), highlighting that IF scores are driven by only a small handful of high-impact articles, raising doubt about their utility as a quality proxy for journals or scholarly papers. Nonetheless, it is unquestionable that the league tables that rank journals by IF began to matter and have become embraced by administrators. It is perhaps not all bad. Data on publications in “high quality” journals could be used by previously marginalized scholars (e.g., entrepreneurship researchers) to make claims about the quality and worth of their scholarship in the academy and at their universities. As a result, previously stable pecking orders could be challenged.

However, there are also costs to such rationalization. Efforts to make individual scholars and universities more accountable through quantitative measurement and rankings (e.g., the U.K.’s Research Assessment Exercise, started in 1992, is now the Research Excellence Framework) have had many negative implications for us as scholars, including

what Greenwood (2016) called formulaic conformity. Formulaic conformity refers to how the ambition of articles in our top journals has narrowed, and how editors and reviewers seem to valorize formulaic presentation of arguments and evidence (e.g., the Gioia method) over appreciation of the content and quality of submission. Corbett, Cornelissen, Delios, and Harley (2014, p. 6) opined, “The voice of the individual in scholarship may become muted to the point where articles in a journal look alike and sound alike, and appear to have been researched and written by the same person.”

In addition, some journals have tried to jack up their IFs by engaging in efforts to increase citations to articles in their journals (e.g., Reedijk & Moed, 2008). For example, Wilhite and Fong (2012) documented how IFs have created incentives for editors to coerce authors to gratuitously add citations to articles that are to be published in their journals. Baum (2011) noted other well-known ways in which journals game IFs, such as publishing more review articles (which are known to attract more citations) and publishing more editorials and book reviews (whose citations jack up the IF numerator but do not count in the denominator since they are not coded as peer-reviewed articles).

Furthermore, the embrace of IFs has also facilitated more blatant forms of misconduct. For instance, in 2014, SAGE Publications announced the retraction of 60 articles implicated in a peer review and citation ring at the *Journal of Vibration and Control* (SAGE, 2014). Among citation problems identified, it was found that an author had created various aliases on SAGE Track and was able to review his own submissions. In 2007, Thomson Reuters began banning journals from their Journal Citation Report rankings for excessive citations or citation stacking, which involves conspiracies between journals to cross-cite each other to enhance IFs. While only nine journals were banned in 2007, the number of journals suppressed has grown dramatically; in 2014, 39 were suppressed. Since Thomson Reuters bans only extreme outliers, this is just the tip of the iceberg.

While those who create and advocate for quantitative scoring may be well intentioned, there is no doubt that the current romance with IFs has been gravely problematic. As suggested above, these are profound problems, bound up in the wider processes of commensuration, the rise of market logics, and related trends such as financialization (Davis, 2009; Thornton, Ocasio, & Lounsbury, 2012). Thus, avoiding the pitfalls of citation abuse in academic journals, while important, is just one manifestation of a broader institutional assault. The broader problem lies with the great violence marketization has

done to pluralistic societal values, narrowing how we assess worthiness across all societal domains. In the context of universities, this has led to a devaluation of the humanities and social sciences in favor of the valorization of more instrumental domains of knowledge that can be shown to concretely matter to short-term outcomes such as economic development (Berman, 2012). How we convince key stakeholders about the value of the liberal arts as well as our individual scholarship goes hand in hand with appreciating a broader set of societal values and ways of evaluating (Barley, 2016).

Despite the enormity of these challenges, we must strive to make our daily practices of “production” and “evaluation” more humane. Can we resist counting and emphasize quality in a more multidimensional way? Can we reform tenure and promotion evaluations to encourage actually reading and discussing a candidate’s work? Perhaps the old-style devil’s advocate system employed at Harvard is worth considering (Fandos & Pisner, 2013). To promulgate a progressive agenda for change, it seems that dismantling the current IF regime might be a useful starting point.

### **Ivory Tower, Value-Free Ideal, and Responsible Science, by Anne Tsui**

I seek to understand scientific misconduct in management research by focusing on the idea of the “ivory tower” and how it is related to the “value-free ideal” in science, to explore the nature of ivory-tower-style research in business schools and to show how the ivory tower actually restricts rather than facilitates academic freedom and independent scientific inquiry. I observe that the ivory tower in the business schools is shrouded by commercial and ideological values associated with entrepreneurialism at both the school and the scholar levels. In this research ecosystem, faculty-scholars have lost the freedom to pursue meaningful research and have missed the opportunity to contribute to a greater good (Tourish, 2011; Tsui, 2009, 2013, 2016). With the primary purpose of producing papers in a prespecified set of journals, and in the absence of a bottom line, temptation to use any means of achieving publication success is great, including even scientific misconduct. Fortunately, we can be hopeful the problem may be solved soon because of the increasing attention on this problem in the past two decades and because of the efforts introduced recently by several leading management journals (e.g., Bettis, Ethiraj, Gambardella, Helfat, & Mitchell, 2016; Meyer, van Witteloostuijn, & Beugelsdijk, 2017) specifically targeted at the problems of questionable (in fact irresponsible) research practices.

The “value-free ideal” in science (Churchman, 1948; Lacey, 1999; Levi, 1960; Reichenbach, 1951) was designed originally to protect scientific work from the interference of social and political values unrelated to scientific process or epistemology. Proctor (1991) gave a detailed historical account of when, how, and why value neutrality was introduced to protect science from the interference of politics and religion. With neutrality, scientists can discover knowledge objectively, unbiased by context (Douglas, 2009; Kaplan, 1964; Proctor, 1991; Risjord, 2014). By being insulated from the worries and social ethos of the larger society, scientists can maintain autonomous inquiry and independent thinking. This separation “permits the individual scientist to concentrate his attention upon problems that he has good reason to believe that he can solve” (Kuhn, 1996, p. 164). According to Kuhn, the autonomy of inquiry has led to impressive progress and success in the natural sciences.

However, uncertainty is inherent in scientific reasoning and inference. Social and ethical values are necessary in judging the sufficiency of evidence when looking to accept or reject a hypothesis and to consider the risk of wrongful conclusions (Rudner, 1953). More important, value freedom does not mean immunity from accountability. At a basic level are simple ethical demands such as honesty, openness, and integrity. Science “cannot succeed unless results are honestly reported, unless every reasonable precaution be taken to avoid experimental error, unless evidence running counter to one’s own view is fairly handled” (McMullin, 1982, p. 7). The value-free ideal is not simply a philosophical issue. It underlies the tension between academic freedom and the social responsibility of universities. Can academic freedom (guaranteed in part by the value-free ideal) justify absolving university researchers’ responsibility to address society’s urgent needs (Bok, 2009)? Should scientists be held responsible for unreliable knowledge or knowledge that may potentially harm (as history has shown) humanity (Douglas, 2009; Proctor, 1991)? There is no clear consensus on the answers to these questions.

In reality, the ivory-tower style of academic research evolved over time to mean that scientists do not need to be concerned about solving real-world problems. In due course, “pure science” or “basic research” became a legitimate reason to focus on problems that are of interest only to scientists. This development occurred in business schools also even though business is an applied discipline, meaning that its knowledge should focus on solving problems of practice, similar to engineering or medicine (Khurana,



2010). Around the ivory tower, an impenetrable wall keeps outsiders from accessing the mystery within. Many scientists in business schools either do not care or merely pretend to care about the practical value of their work. Their research fills journal space and books that only other scientists working on the same esoteric problems can understand or read. Occupying the inner circle are the successful scholars whose work appears in the exclusive, mostly A-ranked journals. Neophytes admire them as definers of theoretical and methodological rigor. Junior scholars must follow their methods and standards or risk being rejected by the prestigious journals. Nonacademic outsiders (managers and the public), though mystified by and even dissatisfied with what happens inside the ivory towers, do not have the scientific expertise to question why or to whom such research is valuable (Kepes, Bennett, & McDaniel, 2014).

Despite occasional critique by the media, business schools lack “a powerful constituency pressing for actionable research” (Pearce & Huang, 2012, p. 258). For example, in the years following the 2008 financial crisis, the top management journals were essentially silent about this issue (Starkey, 2015). Writing papers for the top journals is extremely demanding and highly competitive, so management scholars tend to focus on familiar problems and make contributions in the neighborhood of extant theories and concepts (Barkema, Chen, George, Luo, & Tsui, 2015; Tsui, 2009). After all, they are not rewarded for solving practical problems but rather for the numbers of papers published in certain journals (Adler & Harzing, 2009; Aguinis, Shapiro, Antonacopoulou, & Cummings, 2014). The aspirational agenda of business schools supported by commercial and ideological value shrouds the ivory tower. Instrumentality rather than scientific rationality is the dominant ideology.

This condition suggests that the pure science conducted in the ivory tower is not so pure after all. Its purity can be challenged on multiple fronts. Because it is disconnected from the practical problems of business, financial and intellectual resources are spent on problems that may have neither intellectual nor practical value. This indulgence is not a responsible use of a university's or society's resources. Because they do not have direct contact with practice, the scientists may have incomplete, biased, or even wrong understanding of the problems. They may propose and test solutions for solving the wrong problems, or they may offer the wrong solutions to the right problems.

Consultants, practitioners, or students may believe that the conclusions published in the prestigious or highly ranked journals are valid. But if the research

results are questionable, if solutions and conclusions are of doubtful validity, real-life applications can misguide practice so that “bad management theories are destroying good management practices” (Ghoshal, 2005, p. 75). Because ivory-tower-style research is not meant for practice, business school researchers ignore the problem of “inductive risk” (Douglas, 2009; Hempel, 1965; Tsui, 2016)—wrongful conclusions due to type I or type II error, and whether their findings are replicable (Bettis et al., 2016; Lewin et al., 2016). Consequently, much published work on organizational management may be fraught with wrong conclusions (Davis, 2015). Isn't the lack of attention to wrongful inference a serious form of scientific misconduct?

Societies entrust scientists with the task of explaining the unknowns and solving the puzzles in our natural and social worlds. The expectation is that scientists develop valid and reliable knowledge, minimize errors in their conclusions, and ensure that scientific knowledge, when applied, will benefit and not harm humanity. As Albert Einstein said, “To make life better for ordinary humans must be the chief objective of science” (Isaacson, 2008, p. 374). Social scientists have an added responsibility. “For better or worse, the social sciences are part of the process of social change” (Risjord, 2014, p. 53); that is, social scientists are agents of change, whether they want to be or not. Simply observing can cause unexpected or unintended changes in the social phenomena or the subjects (individuals and groups) being studied.

Due to the authority of science and its potential impact on society, scientists are expected to be responsible experts and public servants who ensure that scientific knowledge is reliable and can beneficially guide practices and inform policies. Fortunately, there is a movement calling for responsible science in the natural science disciplines, including medicine, psychology, and economics, promoting transparency, open sharing, and reproducibility.<sup>3</sup> Similarly, a community of social scientists representing multiple disciplines in the business schools is calling for responsible research in the business and management field (Community for Responsible Research in Business and Management, 2017).<sup>4</sup>

Responsible research will restore scientific integrity by producing credible and useful knowledge for societally beneficial purposes, and will provide true

<sup>3</sup> See the Center for Open Science ([www.centerforopenscience.org](http://www.centerforopenscience.org)).

<sup>4</sup> Researchers can sign up to join this movement at [www.rrbm.network](http://www.rrbm.network).

autonomy of inquiry to the faculty scholars. Responsible science recognizes the importance of both epistemic and social values, both basic research in the ivory tower and applied research on important problems in practice, and the need to replace the “value-free ideal” with “thoughtfully developed values.” Tsui (2016) offered some ideas on how to identify the social and ethical values, along with epistemic values, that are appropriate to guide responsible research in business schools.

A transformation from instrumental research to responsible science is necessary to ensure that business and management research lives up to the purpose of true science, and to ensure that faculty scholars are responsible public servants of knowledge. Through responsible science, business schools can truly become positive forces of change in our highly dynamic, complex, and challenged business and organizational world, and contribute to the creation of a sustainable future for humanity as a whole.

### **Scientific Misconduct as the Normalization of Deviance, by Mary Ann Glynn**

Of late, we have seen concerns mounting about scientific misconduct in management research. It has been cast as an alarming problem (e.g., Clair, 2015; Honig & Bedi, 2012), ripe with “eleven different types of questionable research conduct, including data fabrication, data falsification, [and] plagiarism,” among other concerns (Bedeian, Taylor, & Miller, 2010, p. 715). Blame for such misconduct has been attributed at times to suspect authors and at other times to the publishing process itself, where misconduct “does not require fraud by researchers or excessive sloppiness on the part of reviewers and editors. Rewarding scholars for publication per se, abetted by standard processes of motivated reasoning, is sufficient” (Davis, 2015, p. 181). In other words, the design of the publication system itself promotes misconduct, compounded occasionally by the misdeeds of wayward authors.

Thus, explanations for scientific misconduct range from individuals’ “unfortunate incidents” to the system as a whole, given the nature of the profession and its attendant “normative crisis” (terms used in Honig and Lampel’s 2016 proposal to AMP for this article). In this commentary, I would like to explore an explanation for scientific misconduct that is counterintuitive: that the very systems we design for scientific conduct cultivate the possibilities for scientific misconduct. Our systems can—and do—sometimes fail us for reasons that are endemic to the system.

The specter of scientific misconduct is one that has long haunted scientific inquiry. Early on, the

prescribed corrective was a cultural one, embedded in the institutionalized norms of the profession; one of its most influential proponents was Robert Merton, as Honig and Lampel pointed out. In his 1942 essay, “The Normative Structure of Science,” Merton noted that, because scientists were less subject to regulatory or governmental controls, they relied on normative control, adhering to those taken-for-granted, commonly understood cultural beliefs or values that set expectations about what practices were appropriate or legitimate in research. The irony of this approach, however, is that reliance on the kinds of norm-based systems that Merton describes can enable the very behavior it seeks to disable. Cultural sanctioning can function to normalize deviations such that initial errors go uncorrected and, over time, become the “new normal.” On this point, concerning the unintended effects of normative control and cultural beliefs, sociologist Diane Vaughan’s (1997) landmark research investigating the roles of risk and safety norms in the U.S. space shuttle Challenger disaster of 1986 is instructive. She found that it was “mistake, not misconduct,” as she explained:

Initially, it appeared to be a case of individuals—NASA managers—under competitive pressure who violated rules about launching the shuttle in order to meet the launch schedule. . . . After analysis I realized that people conformed to “other rules” than the regular procedures. They were conforming to the agency’s need to meet schedules, engineering rules about how to make decisions about risk. . . . [T]hey established a social normalization of the deviance, meaning once they accepted the first technical anomaly, they continued to accept more and more with each launch. It was not deviant to them. In their view, they were conforming to engineering and organizational principles. . . . I concluded it was mistake, not misconduct. (ConsultingNewsLine, 2008)

Vaughan’s critical insight was to show how errors are built into all human systems. Over time, slippage in standards occurs without intention or fraud; the result is that such deviance from the established rules can become normalized, over time and over incidents. Other researchers have observed that scholars today are subject to an abundance of pressures that arise from competing institutional arenas, including their professions, their universities, their positions in reputational markets, and aspirational career ladders—each associated with sets of “other rules” that can redirect scientific conduct over time such that deviations can become normalized. Looking at scientific misconduct through this lens highlights how systemic effects contribute to scientific misconduct; that they do is perhaps

not surprising. What seems surprising, however, is the relative lack of systemic counterpressures that might reorient the publication process such that the effects of norm deviations do not become standardized or become rationalizations for misconduct.

To illustrate the role of deviant normalization, we can draw insights from Steve Barley's vivid description of his own career arc and the institutional shifts in research and publishing processes he has witnessed. In a reflective essay, he wrote:

We are well into an era in which academic worth is judged not just by whether you publish or even by how good your research is, but also by where you publish, how many times you publish, and how many people cite your work. . . . [M]aybe it is because I was trained at MIT when being part of the "mainstream" was seen as something of a sell-out and what mattered most was the quality of one's empiricism—but I don't remember this kind of talk when I was a graduate student. . . .

We thought that having some publication was certainly better than none, and the goal was to produce high-quality papers no matter what the outlet. Since that time nearly 30 years ago, school after school has adopted a system for rating journals (the A's, B's, and C's) with the explicit expectation that getting tenure will hinge, at least in part if not largely, on the relative number of A-publications a researcher obtains. . . . Note, then, that the problem with incentives starts before there's even a paper to cite! . . .

In the intervening years, reviewers' expectations and demands have changed. They have done an about-face. I rarely receive any comments these days on my findings, my data, or my analysis. In fact, I am usually complimented on these before being told why the paper can't be published as is. Instead, the vast majority of comments focus on the theoretical or substantive frame of the story I want to tell. The logic of such comments boils down to this: "You say your paper is about X, but I think it is really about Y." Insisting that a paper adopt a framework different than the one the author prefers makes sense only if the framework better organizes the data. (Barley, 2016, p. 5)

Barley (2016, p. 4) maintained that to change this situation, "we have to change our institutions." More specifically, he argued that "if you want to intervene in a system, you eventually have to attack or at least undermine the pilings that support it." And so, it seems, we need to dismantle the current pilings that buttress our institutions that publish scholarly research while simultaneously building ones that support and applaud the norms of appropriate and scholarly research. Tweaks at the margins simply will not shield us against misconduct; rather, what

we need are more fundamental changes in the systems that review, publish, and evaluate our research.

Taking Vaughan, Barley, and others seriously gives us pause—and perhaps a moment or two of pessimism—but also a path forward. Ultimately, patching the system is not going to bring an end to scientific misconduct, nor is adding regulatory controls to the existing normative controls; such solutions fall short of bringing about the kind of fundamental reorientation needed. The problem cuts much deeper, and yet it is, ironically, one of our own making. And so, hopefully, the solution is one of our construction; we can remake our systems, reset expectations, and reinvigorate our research ethos because they are ours.

When we, both individually and collectively, assume our varied roles that cast us as authors, reviewers, editors, and other players who enable the process of producing scholarship, we need to adopt a stance of mindfulness (Weick, 1995) that can cultivate the kinds of cultural expectations, norms, and strategies of action (Swidler, 1986) that are sensitive to both the detection of errors and to the development of workable corrections to improve scientific conduct. And, when we are active consumers of research, perhaps in crafting our own work or in the service of evaluating others for promotion and tenure, mindfulness is again of use. As Barley's reflection on his own career suggests, we need to avoid judgments that emphasize a model of scholars as the "quantified self" of scholarship that emphasize "counts" or numbers, of publications, A-level journals, citations, etc., to the neglect of everything else. We need to redirect our judgments of worth to the substance of what truly matters: the qualitative substance of our scholarship in its own right, as well as its potential for impact on managerial practice, public policy, or our students' learning. This, of course, is a bigger mission and one that may ultimately require a drastic overhaul of some of our cherished institutions attending publishing, university life, and careers; it is a clear and daunting challenge, but one that we need to aspire to, and advocate with, our scholarship.

## ESSAYS EXAMINING MISCONDUCT UP CLOSE

A number of the responding scholars examined misconduct using a more micro-oriented approach, highlighting specific instrumental aspects of our field that either acquiesce to or encourage various aspects of scholarly misconduct. Elke Schüßler is one such example, taking the reader through the ordeal of an emerging European scholar facing the

contrasting influences and micro-processes that socialize young academics. Schübler carefully outlines the cultural change process whereby traditional European scholarship is challenged by a global model through normative scholarly conferences and metrics. Schübler argues that overregulation and overzealous ethics review board requirements have led to a dysfunctional system, such as when rules demand that every conference paper be unique and novel. Schübler observes a conflict between teaching doctoral students ethics and simultaneously teaching them how to strategize the publication system.

Joel Baum takes a systematic and quantifiable approach to the micro-elements of scholarly ethics. Baum points out that turning the focus away from measuring quality toward measuring quantity (measured by citation hits and impact factor) may itself encourage deviance as scholars rationalize their research process. This diffuses into the type of research we conduct, whereby statistically significant results lead to prestige and advancement, thus encouraging the pursuit and selection of possibly biased scholarship through manipulation.

### The Role of Socialization for the Transmission of Norms, by Elke Schübler

**Encountering entrepreneurialism.** My first encounter with the logic of entrepreneurialism in academia was at the first European Group for Organizational Studies (EGOS) Ph.D. workshop I attended in 2006. I was one year into my doctoral studies at Freie Universität Berlin and had not yet thought about publishing and career strategies. At EGOS, we learned about review processes, citation clubs, and strategies different scholars used for maximizing their publication outputs. We saw aspiring scholars as well as those already gleaming in their success. We felt bewildered but also invigorated—there was a game, and we were about to enter it!

A few weeks later, this knowledge began to feel like a burden. I could no longer think about my thesis without feeling frustrated by fears that it would not become a citation hit. Written as a German monograph, my thesis, which was eventually published by a reputable academic publisher and won a couple of prizes, had little value for my career. It would have been much smarter to write a cumulative thesis, in which my thoughts and data could have been streamlined and sliced so as to make particular contributions. Why didn't our supervisors push us down this path? They knew the game well enough!

In retrospect I must say that I remain thankful for having gotten a taste of the “traditional” approach to doing research: being driven by curiosity and interests, theoretical questions, empirical problems—not by strategizing about gaps and impact factors. Today, most doctoral students in the field of management in Germany are socialized in a very different manner: doing quantitative research about an *en vogue* topic that can be published quickly and cumulatively in high-impact journals. Academic books are considered as *Regalmüll*—that is, “shelf trash.” Many job advertisements for management professorship positions in Germany often explicitly call for quantitatively oriented scholars. So instead of allowing for a diversity of intellectual and methodological traditions, the socialization of doctoral students focuses on efficiency and calculable outputs. These doctoral students may not even perceive a clash between communitarian and entrepreneurial demand, because they have never encountered Merton's (1942/1973) logic of communitarianism and disinterestedness in the first place.

**What's your score?** The widespread use of journal rankings in recruitment decisions and evaluation procedures underlies this shifting logic. The German business studies scholars association came up with its own ranking, JOURQUAL, which is often used as a basis for calculating an applicant's “publication points.” Another commonly used ranking system is the Handelsblatt ranking, in which a calculation-based rule for penalizing coauthorship is used. The outcomes of these procedures can be felt directly in collaborative research projects, where it is increasingly challenging to maintain a communal spirit and norms of reciprocity regarding the group, especially as coauthorship is systematically punished.

The journal ranking systems also define the priorities for recruitment decisions, making it more difficult for recruitment commissions to consider other legitimate criteria such as nondiscrimination, creating a family-friendly organization, or recruiting a team player who fits best into the faculty. These problems can be illustrated by a recruitment process I have been involved in. At the end of my postdoc period I was competing for a job with a colleague who had finished his thesis around the same time as I had but hadn't yet had children, resulting in a longer publication list. In the light of otherwise very similar qualifications and achievements, several senior academics tried to objectify my maternity leave period through all sorts of calculations, with some coming to the conclusion that, even if I hadn't been on leave, I would have had fewer publication points than my

colleague—who in procedures like this inevitably becomes a competitor; others argued that we would be equally qualified.

After a painful process, the commission gave my competitor the offer. His performance was real, whereas mine was potential. When the diversity officer questioned this choice, suddenly everyone in the commission unanimously agreed that he was the far better candidate. Such procedures, which are based on the idea that decisions can and must be objectified, systematically discriminate against scholars—men and women—who have taken up care duties, and it is worth reflecting on how this affects not only scientific conduct but also the kind of knowledge we generate (cf. Parker & Weik, 2014). It must be added that there was a happy ending for me in this recruitment process, as eventually gender equality funds were used to also make me an offer—highlighting the importance of such measures to counterbalance some of the market forces inherent in ranking-based evaluation criteria. At least in the German-speaking countries, such funds or other equality measures are not yet available to the same extent for supporting ethnic minorities or people from disadvantaged social backgrounds, who might face their own obstacles to measuring up quantitatively with potential competitors.

***Collaboration—despite journal rankings and policies?*** The journal ranking system also creates obstacles for interdisciplinary collaborations, because most journals from other disciplines simply do not appear in the German rankings. It depends on the whim of individual recruitment and evaluation commissions to include such journals when calculating the points for a particular scholar, creating clear disincentives for junior scholars to publish in interdisciplinary journals. This problem is exacerbated by the limited availability of tenure-track positions in Germany, where a growing number of junior scholars on precarious contracts compete for a shrinking number of full professorship positions.

Additionally, business studies journals are now—not least in light of recent retraction scandals—particularly strict regarding the repeated use of data in different publications. Such rules, in my view, create further problems for scholars seeking to do what is often considered “relevant” research (e.g., by collaboratively investing in unique data collection rather than using an already existing data set). Interdisciplinary and collaborative research is thus not only more work- and time-intensive because of the necessary “translation work,” but also potentially unrewarding for management scholars. So far I have not strategically picked my

collaboration partners based on these factors, but I may do so in the future given these obstacles to interdisciplinary and even international collaboration. Is this what we wish the future of independent scholarly research to be?

***Closing the iron cage and unintended consequences.*** I would argue that ethical concerns have so far led mainly to an overregulation of an already dysfunctional system, thus reinforcing an already problematic path. Does it really make sense to consider, as the Academy of Management conference does, the submission of a paper that has already been presented at another conference a form of “self-plagiarism,” given that papers are being developed over long periods of time in ongoing dialogue with the scientific community? What notion of “originality” and academic progress does it convey if each and every submission to a conference needs to be something completely novel? Is it realistic to assume that scholars can come up with new data and ideas for every conference visit, given obvious time constraints and the fact that papers are in review processes for years (and given that some paper presentations are attended by not more than five to 15 researchers)? And, again, what about interdisciplinary research projects, where similar ideas are presented to different audiences? Do such rules not encourage even further salami-slicing and commonly accepted unethical behavior such as playing around with titles to hide the content of a paper that has been presented elsewhere?

Similarly overregulated are the ethics approval systems that many Anglo-American universities have implemented to control the risk of litigation. For example, in an international research project, my collaborators and I aim to share data across countries for comparative analysis. The ethics approval board at the institution one of my partners is affiliated with has asked for formal consent from each interview partner that data be sent to whichever country for final analysis. Such regulations might eventually exacerbate moral problems by making qualitative research—typically less subject to data manipulation, data stealing, first mover advantages, etc.—increasingly difficult to do.

Some have suggested that introducing ethics courses into doctoral education will help to reinforce scientific norms (Honig, Lampel, Siegel, & Drnevich, 2014). However, I see a risk that such a measure would be a similar addition to the iron cage that maintains and even worsens, rather than fundamentally questions, an already dysfunctional system. To preach ethics while at the same time

teaching doctoral students how to best strategize their way around the publication system may rather serve to legitimize our current system and encourage a cynical stance toward research ethics.

**Counterforces.** As research on the regulation of professions has shown (Quack & Schübler, 2015), national-level actors can be a strong force in challenging globally converging trends. In Germany, the national business studies scholars association has been leading a critical debate about journal rankings and their mechanistic use in the German academic career system (e.g., Kieser, 2012). Such efforts are promising and should be reinforced, as they can contribute to delegitimizing current practices.

### Counting on Dishonesty, by Joel Baum

**One bad apple.** The recent retraction of 16 articles published in some of our field's most prestigious journals—*Academy of Management Journal*, *Journal of Management Studies*, *Organization Science*, *Research Policy*, *Strategic Management Journal*, and *Strategic Organization*—has raised the specter of academic dishonesty within the Academy of Management.<sup>5</sup> It may be comforting that one “bad apple” is implicated in all of these retractions. Indeed, beyond this scandal, instances of academic misconduct appear uncommon in our field (Karabag & Berggren, 2012) compared to the life sciences, for example, where such retractions number in the thousands, many more of which are attributable to misconduct than error (Fang, Steen, & Casadevall, 2012). Strong moral character is unlikely to explain our apparent fidelity, however. It is more likely that we invest less to detect, and perhaps are more reluctant to publicize, academic dishonesty, even as how we assess research performance and select articles for publication seems certain to encourage almost everyone within our field to cheat, at least a little—but still too much.

**A lot of cheating a little.** Mažar, Amir, and Ariely (2008) cautioned that dishonesty is not about a few bad apples but rather that almost everyone cheats a little. Their idea is that a range of dishonesty exists within which honest people are able to rationalize their actions in a more positive light, allowing them to cheat (a little) while maintaining a view of themselves as honest people. Beyond this range, however, it becomes hard for honest people to justify dishonesty and maintain their self-concept. Fabricating

data or results and plagiarism or duplication may be too much for all but a few of us to rationalize. Other questionable practices that finesse publication, however, appear to be within a range of dishonesty that many of us are able to rationalize:

- Selectively omitting (or including) variables, observations, and/or statistical analyses until non-significant results become significant at standard levels, or “*p*-hacking” (Simmons, Nelson, & Simonsohn, 2011)
- Selectively reporting results consistent with hypotheses in which we are invested (Fanelli, 2010b)
- Presenting post hoc hypotheses as if they were a priori hypotheses, or “HARKing” (Kerr, 1998).

Despite being labeled “deceptive” and “unethical” (Schwab & Starbuck, 2017), these practices still appear as misdemeanors alongside fabrication, plagiarism, and duplication, making them easier to justify in a more favorable light. Unfortunately, honest people who cheat a little every now and then may soon find it difficult to justify their accumulated dishonesty, leading them to give up and cheat a little by default (Mažar & Ariely 2012).

Indeed, such “little lies” (Schwab & Starbuck, 2017) appear commonplace in our field (Bedeian et al., 2010). Distributions of test statistics exhibit too many with *p*-values within a narrow band just below .05 (i.e.,  $.04 < p < .05$ ) and too few to either side of this range (i.e.,  $.03 < p < .04$  and  $.05 < p < .06$ ), a pattern compatible with researchers choosing model specifications that boost the significance of coefficients that fall just below standard levels (Baum & Bromiley, 2017; Goldfarb & King, 2016; Head, Holman, Lanfear, Kahn, & Jennions, 2015). There is also an excess of findings consistent with hypotheses in the social sciences, which report two to three times more positive results than the physical sciences (Fanelli, 2010b, 2012). In management, the success rate for tested hypotheses may be as staggeringly high as 90% (Schwab & Starbuck, 2017).

**Cheating a little to publish . . . or perish.** Academics in many fields lament the willingness to assess researcher performance by counting articles and weighing them by the IF of the journals in which they appear, without certifying the researcher's contribution. This practice—compelled perhaps by the fact that the volume of publications has outpaced our capacity to obtain expert peer judgments, perhaps by the desire to put academic decisions on a more rational, quantitative footing (Baum, 2011)—shifts the currency of recognition and reputation away from publication quality, either in originality and precedence (Merton,

<sup>5</sup> Source: <http://retractionwatch.com/the-retraction-watch-leaderboard>, accessed January 11, 2016.

1957) or use and citation (Hull, 1988), toward publication quantity, particularly in journals where other work is widely cited: “Getting things into print becomes a symbolic equivalent to making a significant discovery” (Merton, 1957, p. 655).

Whether or not a researcher attains the requisite number of top-tier journal “hits” has become decisive in academic careers (Butler & Spoelstra, 2012; Hussain, 2015), rendering publication in prestigious journals an overriding objective (Macdonald, 2015) and researchers more concerned with where than with what they publish (Alvesson & Sandberg, 2013; De Rond & Miller, 2005).

A focus on counting rather than qualitatively assessing the contribution of publications may tempt researchers who fall short of the requisite number of hits to turn to more questionable research practices that offer the chance to reach that number (Charness, Masclet, & Villeval, 2014; Greve, 1998).<sup>6</sup> The salience counting places on upward social comparisons in particular may encourage researchers to cheat to maintain positive self-evaluations (John, Loewenstein, & Rick, 2014). Emphasis on quantity rather than quality also primes analytic, rational (rather than affective, emotional) reasoning processes that facilitate self-serving rationalizations with which to justify their dishonesty by limiting emotions such as guilt (Zhong, 2011).

Reinforced by the editorial review process at journals with high IFs, which rewards statistically significant results that support a priori hypotheses with publication (Head et al., 2015), a focus on counting creates incentives for researchers—abetted by powerful computers and statistical software—to selectively pursue statistically significant positive findings that support an accumulation of the requisite number of hits (Necker, 2014).<sup>7</sup>

Our field thus rewards statistically significant positive findings with prestigious publications, which are in turn rewarded with research grants, tenure, and promotion in a highly nonlinear manner. These rewards tempt us to cheat a little and, tempted repeatedly, to habitually disregard negative and nonsignificant findings in favor of selectively collecting and analyzing data in a search for significant findings hypothesized after

the results are known, filling our journals with false positives and exaggerated results (Bettis, 2012; Simmons et al., 2011).

**Don’t spoil the whole bunch.** To break this cycle, we will need to coordinate the fragmented activities of deans, journal editors, granting agencies, and academic committees, all of which rely on an even more fragmented peer review process. Like any change effort, the actions of a few respected first movers are required to get us going.

Key journal editors can adopt policies advocating tests of statistical significance less susceptible to manipulation, submission of replication studies and nonfindings to help weed out false positives (Mezias & Regnier, 2007; Starbuck, 2016), clear disclosure of results already published from the same study, and release of associated data, as well as developing policies that broach the limited likelihood and consequences of academic dishonesty being detected that characterize our field (Bergh, Sharp, & Li, 2017; Honig et al., 2014; Lewis, Duchac, & Beets, 2011). Notably, the coeditors of *Strategic Management Journal* (Bettis et al., 2016) announced that, as of January 1, 2016, they would welcome studies reporting replications and nonresults and shift emphasis from statistical cut-offs to coefficient effect sizes, empowering their editorial board, reviewers, and authors to stop favoring statistically significant positive findings for submission and publication, and commit to nurturing the vital roles of replication and negative and non-significant findings in advancing knowledge.

Although welcome, such editorial policies are countermeasures; they acknowledge but only indirectly confront the incentive system we have created—a system at odds with basic norms of science: It inspires neither “disinterested activity” nor “detached scrutiny” (Merton, 1942/1973, pp. 276–277) but rather “excessive concern [on] ‘success’ in scientific work . . . that can lead by gradations from rare practices of outright fraud, to more frequent practices just beyond the edge of acceptability, sometimes without the scientist’s being aware that he has exceeded allowable limits” (Merton, 1957, p. 651).

To confront the system directly, progressive deans, chairs, academic committees, and peer reviewers at key institutions should stop recommending tenure, promotion, and research grants based on more rather than more significant publications, and commit to more careful reading and closer expert assessment. Rather than counting articles, they should emphasize deep assessment of fewer papers, perhaps limiting the number to three or four (maybe five), recognizing both

<sup>6</sup> Researchers with shorter publication histories are more likely to author retracted papers (Fanelli, Costas, & Larivière, 2015).

<sup>7</sup> The higher proportion of fraud found in prestigious journals is consistent with the benefits of publishing in such journals being an incentive for dishonesty (Fang et al., 2012).

our limited capacity to obtain and render expert peer judgment and the harmful incentives that pushing researchers to overpublish creates. If they do attend to citations, they should pay more attention to articles than to journal citations, recognizing that publication in a high-IF journal does not make a paper good (Baum, 2011; Starbuck, 2005).

If we can come together on commitments such as these, we can begin to dismantle the incentives that tempt us to cheat a little, which, unchecked, will undermine the veracity of our research and the scientific integrity of our field. I ask you to consider how you will contribute to ending our counting, to rewarding our honesty, and to reasserting our integrity.

### ESSAYS SUGGESTING SOLUTIONS TO MANAGEMENT'S ETHICS DILEMMAS

Last, we explore the essays that suggest remedies and offer ideas on how our practices, if not the entire system, should be reformed. In the previous essay, Joel Baum looks to early adopters, such as deans, editors, and academic committees, to take the lead in breaking the cycle of statistical significance. Recognizing that recent developments in certain journals (e.g., *SMJ*) improved ethical practices led by progressive deans, chairs, and committees would introduce new incentives focusing on quality rather than quantity, welcoming replications, nonresults, and nonsignificant findings that lead to advances in knowledge.

Arjen van Witteloostuijn takes a more specific technical perspective, contrasting the inappropriateness of 19th-century academic processes that were far more personalized with 20th-century technology that has led to scarcity and monopolization. Referencing the scientific Wikipedia model, Witteloostuijn argues for a grass-roots movement to uphold publication and editorial standards<sup>8</sup> in an effort to develop a dialogue leading to better practices.

David Sirmon focuses on specific actions the field should take to reduce academic misconduct and make our research techniques more transparent. He suggests "methodological recipe cards" for authors to provide more information to reviewers and editors. Sirmon also asks all journals to begin addressing issues of questionable research practices through academic misconduct statements, to begin a dialogue examining practices and norms and enhance our overall integrity.

Finally, Runtian Jing approaches the diffusion and rise of entrepreneurialism in management research

as a merger of economic and reputational interests, with rapid growth in many of the emergent economies such as China. Arguing that failing to focus on ethics is unsustainable and leads to misconduct, Jing advocates for a model that is collaborative and communal, while also emphasizing indigenous research that addresses the unique contexts of different environments. This might be done with different forms of accreditation, as well as with more rigorous social control systems that regulate scholarly misconduct.

### Toward a Scientific Wikipedia, by Arjen van Witteloostuijn

**A two-fold crisis.** This little essay is primarily, but not exclusively, directed at the worldwide business and management scholarly community. I passionately believe that our discipline, or set of disciplines, must deal with a number of deeply rooted problems that can be summarized as the twofold publication bias and replication defect crisis. We, as a collective, violate basic scientific principles by (a) mainly publishing positive findings (i.e., those that are in support of our hypotheses) and (b) rarely engaging in replication studies (being obsessed with novelty). Clearly, business and management is not the only discipline in crisis; quite the contrary (see, e.g., John et al., 2012; Melander, Ahlqvist-Rastad, Meijer, & Beermann, 2003). But the least we can do is try to clean up our own mess.

It is not that we are unaware of this—not at all. For instance, in 1996, Hubbard and Vetter reported that replication studies were rarely published in 18 top journals in accounting, economics, finance, management, and marketing; in 2012, Bettis (p. 110) observed that "data snooping or searching for asterisk" is endemic in business and management. As a result, our precious academic journals are filled with false positives (Ioannidis, 2005). Why this dismal state of affairs continues to corrupt our scientific community is also very well known: With all our journals seeking to publish "groundbreaking" and "cutting-edge" research, and with our institutes' HR policies grounded in impact factor metrics, what else can we expect (Birkinshaw et al., 2014; van Witteloostuijn, 2016)?

**The digital revolution.** On top of the counterproductive internal dynamic that dominates our scientific community, we have to deal with an external change that is likely to radically shake up the way we play the research and publication game: the digital revolution. The way we publish is still heavily rooted in practices developed in the 19th century, using 20th-century technology. Back then, academic

<sup>8</sup> <https://www.change.org/p/the-scientific-community-change-the-way-we-conduct-report-and-publish-our-research>



journals were established to open up and scale up the basically dyadic and rather private communication between scholars, who used to exchange bilateral and handwritten personal letters. With the help of publishers, this was turned into the double-blind reviewing mechanism that is still in use today, in 2018, aimed at selecting papers that are deemed worth publishing on the printed pages of a limited set of academic journals. Although online and early publication are common practice by now, and notwithstanding the emergence of open-access outlets such as the PLoS journals, the core of modern practices has hardly changed at all. Given the twofold crisis in academia, and in combination with the opportunities offered by quickly progressing digital technologies, this outdated practice is no longer sustainable.

**Dynamic and open publishing practices.** Current publication practices are stimulating publication biases, triggering malpractices, and harboring closed shops. This equilibrium is stable due to the monopolizing tendencies associated with scarcity. However, this scarcity is rooted in the old technology of paper-based printing. In the world of cloud computing, publication space is, in principle, close to infinite. Moreover, cloud computing technology offers ample opportunities for free entry and costless exit, implying that the market for academic publishing is potentially a perfectly contestable place. What frustrates entry are established and highly inert practices that protect the vested interests of incumbent academics, institutes, journals, and publishers. However, cloud computing offers a gateway to entry by scholarly entrepreneurship that could radically change current research and publication practices to the benefit of our academic community at large. This little essay is not the place to extensively argue what such radical change could look like. Hence, an illustrative example of what may be referred to as scientific Wikipedia has to suffice.

**Scientific Wikipedia.** Wikipedia is a free encyclopedia built collaboratively using wiki software. Similarly, a scientific Wikipedia (SW) could be developed dynamically, openly, and collaboratively by the scholarly community. After an initial screen by an editorial board, any submission that passes a minimum threshold of scientific rigor could be posted on SW without any immediate need for changes. Subsequently, reviewers would be asked to write and post nonanonymous comments. Moreover, all SW-reading scholars would be invited to post nonanonymous comments. At any time, the authors could decide to upload a revised version of

their original paper, to withdraw the original study, or to write a separate response. To each accepted submission, a dynamic metrics account would be attached, providing a series of statistics (number of times cited and downloaded, number of revisions, links to papers citing this submission, etc.), similar to that offered by Research Gate. Additionally, not constrained by space limitations, SW could adopt a series of practices that counter the twofold crisis. Examples are separate sections for meta-analyses and replication studies, a preregistration repository, a dedicated replication team, and material upload requirements. SW would be free, not charging any fees, and strict open-access policies would be pursued.

**Institutional inertia.** If there is one stylized fact in the social sciences, it is this one: Changing an inert institutional system is very hard indeed (cf. Hannan & Freeman, 1977; Hayward, 1976). So introducing SW is one thing; having SW accepted as the new standard is quite another matter. The key issue is that a mountain of vested interests is standing in the way of change. Incumbent academics have grounded their careers, prestige, and power on these 19th-century practices. Academic publishers will do everything they can to protect the huge profits from the scarcity of publication space. Academic institutes have developed a complex and subtle architecture of incentives and HR practices that are rooted in impact factor metrics. Et cetera. What makes matters worse is that the academic community hosts large variety across and within different stakeholder groups. To mobilize such a heterogeneous tribe of stakeholders to engage in forceful collective action is anything but easy (Olsen, 1965). However, the shore may well stop the ship.

**Grassroots movement.** Many share the feeling that we have to change our old ways. Examples of the signs of changing times are manifold, as is clear from new outlets such as the *Academy of Management Discoveries* and the *Journal of Business Venturing Insights*, the change in reporting practices announced by the *Journal of International Business Studies* and *Strategic Management Journal* (see, e.g., Meyer et al., 2017, and Bettis et al., 2016, respectively), the plea for responsible science in the AoM community, and the hybrid registered reports submission path launched by 10 organizational behavior journals (e.g., *Leadership Quarterly* and *Organizational Research Methods*).

If you share the worries and dreams expressed in this little essay, I would highly appreciate it if you could explicitly signal your support. The follow-up blog associated with this article will be at

<https://ethicist.aom.org> and will be under the heading Reflections on Scientific Misconduct in Management: Discussion Space. To kick-start this dialogue, above and elsewhere (cf. van Witteloostuijn, 2016) I provide a tentative suggestion regarding a new way of publishing, for now referred to as a scientific Wikipedia. My hope is that by initiating this dialogue, a few of the measures suggested above will indeed be implemented, and others—perhaps far more effective ones—will be added in due course. The time is right to start organizing collective action to improve the state of our beautiful and wonderful business and management scientific community. In 2017, the 21st century has finally arrived, offering the opportunity to benefit from the digital revolution to improve the functioning of the academic research community.

### **Toward Transparency in Methodology, by David Sirmon**

The number of scholars “competing” for limited top-tier journal space is increasing (Certo, Sirmon, & Brymer, 2010). Moreover, the rewards for publishing in the top journals are handsome—maybe even more so today than in decades past. It can be argued that these dynamics combine to undermine the long-term scientific norms of the management and organizations community toward academic integrity, promoting a “cutthroat” approach to research and leading to increased incentives for academic misconduct. However, there may be reasons to question this argument. First, few, if any, of us were drawn to the profession in hopes of creating either substantial personal wealth or fame. So, while the financial rewards are attractive and respect among peers is valued, these seemingly have not changed substantially enough over time to singularly motivate increased academic misconduct. Second, in every academic institution I have had even the slightest engagement with the traditional norms are still strongly promoted. We all care deeply about our field, colleagues, and students and take action-based steps to ensure our collective standards. As such, it is my hunch that the base rate of academic misconduct has not changed a great deal over decades; however, given the larger pool of people involved in our collective research endeavor, the sheer numbers of misconduct cases could grow, and that is alarming.

Regardless of which argument is correct, academic misconduct is a threat to our mission: the creation and dissemination of useful managerial and organizational knowledge. And it is my position that we can take two low-cost steps to further strengthen our

norms of academic integrity that may actually decrease the base rate of misconduct. But before discussing those, I think it is worth balancing this discussion against any costs of implementation.

No doubt about it, fraud is fraud. It is willful cheating. We must be vigilant to the prevention of fraud, but let’s not throw the baby out with the bathwater as we address concerns about academic misconduct. In other words, let’s be cautious that any new actions conceived to prevent misconduct are not so onerous that only the well-resourced scholar can engage in the advancement of knowledge. For example, the creation of insightful data sets can be extremely expensive, and thus requiring the public dissemination of said data as a condition of publication might be an overly harsh requirement for many participating in our mission, while for others it is less punitive.

Taking a page from agency theory, we need not assume that everyone is an “agent” to agree that some actions might dissuade inappropriate behavior. Actions in the review process could be quite helpful in deterring any motivation for academic misconduct. First, requiring a concise yet comprehensive “methodological recipe card” for each submission would help reviewers better understand what was done and reported versus what was done and not reported. This methodological recipe card would detail each action taken with the data, and while it might not be needed for the general reader, its inclusion would increase transparency in the review process.

Following the argument of Schwab and Starbuck (2017), I suggest that three topics that are seldom discussed in the methodological sections of published papers be addressed: 1) HARKing (or the development of hypotheses after analysis provides support), 2) *p*-hacking (or the running of many versions of primary models to find the “best” results), and 3) selective reporting (or the reporting of only supported hypotheses but not those failing to be substantiated). With these issues clearly presented, the editorial decision process can be used to make more informed recommendations to the author(s). For example, it might be deemed useful to print and elaborate on nonsupported hypotheses, or the sensitivity to model specification, or how the final model iteratively developed. This brings me to my second step of action: requiring each coauthor to attest to his or her awareness of any issues pertaining to academic misconduct at submission, including the authenticity of the data as well as the transparency of the testing regimen. While neither of these actions will prevent the hardened fraudster, both will aid in the development of a norm for transparency.

Moreover, these steps would move the muddy area of academic misconduct—so-called “questionable research practices” or “covert research practices”—into the light. Authors would clearly shoulder responsibility for misconduct in errors of commission (e.g., data manipulation) and errors of omission (not fully disclosing their methodological process). It is my opinion that the discussion of questionable research topics is spotty. In reviewing the support materials for submission to the *Academy of Management Journal* I found no central treatment of questionable research activities. However, *AMJ* has addressed some activities in forums such as “From the Editors,” and the Academy’s Code of Conduct is helpful although not as specific to many questionable activities. It might be best for *AMJ* and other journals to centralize these treatments in one easily found document. That is what *Strategic Management Journal* seemingly has done. *SMJ*’s current thinking on several questionable research practices is clearly stated in an easily located document titled “Guidelines Regarding Empirical Research.” This document is found on their website under Editorials and Primers.<sup>9</sup>

It would be helpful for all journals to address these topics. While this essay is not the proper forum to discuss questionable research activities in detail, it is my view that some activities often listed under this banner are simply fraud, while others are much less menacing, and based on Schwab and Starbuck’s argument could actually add value if fully disclosed. To strengthen our norms of academic integrity we need to beat back ignorance related to questionable research activities and bring them clearly into the light for discussion. Let’s make our treatment of questionable research practices part of our academic norms, thereby strengthening those norms. Much like the “broken windows” theory of policing, more clear treatment of questionable research practices might lower the likelihood that a researcher would move past them to even more grievous forms of academic misconduct. By publishing and updating a questionable research practices document, top-tier journals might act as a central organizer to further strengthen our collective norms for academic integrity.

Competition for top-tier journal space can be positive as it spurs innovative and ambitious scholars to pursue excellence, but we need not accept academic misconduct as a by-product of such competition. Regardless of whether my hunch that the base rate of

academic misconduct has held steady is correct, two low-cost actions—methodological recipe cards and attesting to the lack of academic misconduct—might prove useful. I am optimistic.

### Management Research in/for a Society, by Runtian Jing

Since its birth, scientific research has set truth seeking and knowledge innovation as its superordinate goal through the integrity of scientists. However, with the transition of the political and social environments that scientific research is embedded in, academic dishonesty and misconduct increase progressively (Martinson, Anderson, & de Vries, 2005; Open Science Collaboration, 2015). In such situations, the mainstream explanations for academic misconduct have changed from a scientific ethos to the reward system and institutional arrangements on which research relies. Some researchers hold the view that Merton’s (1942/1973) normative view has expired due to the tremendous changes in current society. The new entrants to the scientific community do not accept the previous norms, and many political, economic, and cultural factors have been introduced into the scientific process (Mulkay, 1969). Of course, there are many theoretical debates here. For example, Zuckerman (1977) was of the opinion that the increase in academic misconduct exactly reflects the significance of scientific ethos; we cannot look into social norms in an isolated way, because each norm relies on others to build up the integrity of the scientific ethos. Because of the issues occurring in the global management community, especially in emerging economies, I propose that:

- (1) The rise of entrepreneurialism invalidates the intrinsic motivations of management researchers. Today, more and more business schools are eager to use top-tier journal publications to assess research quality, which is enforced by the embedded reward systems (e.g., compensation, promotions and tenure decisions, endowed chairs and professorships). Such a utilitarian institution is externally stabilized by the accreditation institutes in ranking the academic qualifications of business schools. Such narrow definitions of high-quality research cause fierce competition among academic institutes and researchers for limited journal space, resulting in entrepreneurial activities in the management community. Meanwhile, as the self-determination theory predicted (Deci &

<sup>9</sup> <http://strategicmanagement.net/pdfs/smj-guidelines-regarding-empirical-research.pdf>

Ryan, 1985), the short-cut extrinsic incentives between publications and economic rewards can easily invalidate the intrinsic incentives from the beauty of research itself. Without self-consciousness, publication stress magnifies the personality defects of researchers, making hasty misconduct behavior acceptable to them.

- (2) Business schools neglect their supervision and monitoring functions of misconduct behaviors. Unlike in Merton's time, today the economic benefits of researchers are closely associated with the interests of business schools; the schools require fast and productive publication without undertaking the responsibility of cultivating and supervising academic ethics and even overlook some members' misconduct, which could cause feelings of unfairness among researchers within organizations. This is not a sustainable game, just like a company that obtains profits from selling products but is reluctant to undertake the duty of quality control.
- (3) Homogenized research paradigms and weak academic norms cause a higher incidence of misconduct in emerging economies. During the past 20 years, emerging economies in Asia, Eastern Europe, and Latin America began to imitate the U.S. brand management research model. For example, in China, the leading business schools all adopted the "publish or out" policy to tenure their junior faculty, based on the publication quantities in the UT-Dallas or *Financial Times* journal lists. The journal list has also been taken as a criterion point to assess applicants' research capability and achievements for governmental grants and awards. From 2011 through 2015, Chinese scholars published 38 papers (10.4% of their total publications during the period) in the *Academy of Management Journal*, 26 papers (7.6%) in the *Journal of Marketing Research*, and 79 papers (10.0%) in *Management Science* (Yang, 2016). On one hand, this has intensified the competition inside the global research community; on the other hand, it has led to even more misconduct instances in emerging economies due to their weak academic norms (Honig & Bedi, 2012; Zeng & Resnik, 2010).

Based on the above propositions, I hereby offer the following suggestions:

- (1) Follow an engaged scholarship model to seek the meaning of research. The role of science or of the scientist, if not understood properly, may lead scientific work to stray from its essence (Oliver, 2010). Here we need to be careful about the

"interesting story" metaphor of management research. To make the story seem interesting and coherent, researchers may sacrifice their obligations to the truth by tailoring their findings and evidence. As Bedeian, Taylor, and Miller (2010) have reported, approximately 80% of their surveyed researchers were aware of cases in which their colleagues had withheld methodological results or selected only the data that supported their propositions. Such an entrepreneurial view of research may appall us—that researchers can desire, extract, and claim their ownership of knowledge.

Here, to build social collaboration and communal ownership with different stakeholders, researchers should follow an engaged scholarship model, which is defined as a participative form of research for obtaining the views of key stakeholders (academics, practitioners, policy makers) to understand a complex problem in its particular context (Van de Ven, 2007). By exploiting differences in the viewpoints of the key stakeholders, engaged scholarship can contribute knowledge that is more insightful than when researchers work alone, and through which they can act in their academic roles not only in society but also for society.

- (2) Emphasize indigenous research in various assessment scenarios as a fundamental ecological rule—no variations, no adaptations. Our scientific community encourages diversified research paradigms, and the West has no monopoly rights in defining scientific theories, methods, and institutions. Therefore, indigenous research is badly needed to release the homogenizing institutional pressures and to develop management research that expresses and celebrates the unique contexts and settings of different countries and regions (Van de Ven & Jing, 2012).

At present, accreditation institutes adopt comparatively unified academic criteria to rank global business schools, which is an important reason for schools in emerging economies to imitate the Western management research model. As a suggestion, accreditation institutes can design more effective criteria to evaluate the academic qualifications of business schools by recognizing their indigenous contributions to local communities. Moreover, the records of misconduct instances should be included as a negative criterion in accreditation.

- (3) Establish a social control system for academic misconduct. Working on the honor system, the

management community neither supervises researchers' on-site behaviors nor develops a strict control system like that used in other occupations such as medicine and law. Currently, the severity of academic misconduct impels some governments and institutes to take greater measures (Zeng & Resnik, 2010). For example, researchers are encouraged to officially report academic misconduct around them. An investigation by Keith-Spiegel, Sieber, and Koocher (2010) indicated that under such circumstances, 53% of their respondents said they would choose to report misconduct problems to academic institutes, and 39% of the respondents with such experiences were satisfied by the results of impeachment.

In sum, misconduct disgraces the sacred field of scientific research. To solve this problem, both external and internal control methods are needed. Meanwhile, as Albert Einstein said, "Most people say that it is the intellect which makes a great scientist. They are wrong; it is character" (as quoted in Wolpe, 2006, p. 1,023). We believe that only through the integrity of scientists can our tribe of scientists advance its scientific ideas and discoveries about living nature and humanity sustainably.

### CHALLENGING THE ILLUSION OF PROFESSORIAL EXCEPTIONALISM

In this final essay, Jim Walsh acknowledges the role of scholarship through a broader social lens, while reflecting on some of the contributions (and limitations) of the other essayists' recommendations. Taking a macro objective, Walsh first acknowledges the evidence of growing scientific misconduct, but directs our attention to misconduct in other professions, notably jurisprudence and divinity, that base their legitimacy on claims to exceptional moral integrity. Walsh suggests that increasing awareness of misconduct in other fields that make these claims should alert us to the fact that we are subject to the same underlying human frailty and weaknesses as professionals elsewhere. This in turn should lead us to end our complacency, and direct our attention to remedies that reduce and deter misconduct. Walsh highlights some of the progress management scholarship has undertaken in this regard (e.g., AOM's code of ethics) and urges more open debate on the issues aired in this project. But ultimately, Walsh's primary point is the importance of humility in the face of our own fallibility, suggesting that the challenge we continuously face is avoiding the hubris that leads us down paths we all know we must avoid.

### Humility and Human Frailty, by James Walsh

I read Benson Honig and Joe Lampel's letter of invitation with a mix of acute sadness and appreciation. The sadness is self-evident. While no expert on scientific misconduct, I would have to have been a modern-day Rip Van Winkle to miss the recent concern about our dubious—if not bad—behavior (cf. Banks et al., 2016; Bedeian et al., 2010; Goldfarb & King, 2016; Honig & Bedi, 2012; Honig et al., 2014; Karabag & Berggren, 2012; Kepes & McDaniel, 2013; Schwab & Starbuck, 2017). Honig and Lampel returned to Merton's (1942/1973) analysis, if not celebration, of the cultural and moral structure of science (written in the dark days of fascism) to help see us through our own dark days. It certainly is an ennobling read. Like Merton (1942/1973), I would like to think that we are better than this. Indeed, I stood a little taller after hearing him refer to our "scientific conscience" (p. 268), the "moral consensus of scientists" (p. 268), the "humility of scientific genius" (p. 273), and the "integrity of the men [*sic*] of science" (p. 275). In a like-minded moment, I once remarked that we are one of just three professions whose members don honorific robes. We scholars join justices and religious holy people in that practice. Truth is, I believe that our work is sacred (Walsh, 2011).

Nevertheless, we are human. We are each as capable of sin as we are of sainthood. Indeed, sitting for an extended interview upon his election, Pope Francis was asked, "Who is Jorge Mario Bergoglio?" He made news with his simple reply: "I am a sinner" (Spadero, 2013). We do ourselves a disservice if we deny our human frailty and imagine ourselves to be above reproach. Internalized, our understanding of moral licensing tells us that this kind of attitude can breed a reckless arrogance (Blanken, Van de Ven, & Zeelenberg, 2015; Merritt, Effron, & Monin, 2010). In fact, Batson and his colleagues feared that we are all moral hypocrites (Batson, 2008; Batson, Thompson, & Chen, 2002). And so, oddly enough, Merton's (1942/1973) words might even do us more harm than good. Celebrations of our special conscience and humble genius may help us stand a little taller, but they can also bring us down. Embrace such words and we may even find ourselves with few friends to pick us up when we stumble and fall. After all, the bigger we are, the harder we fall.

Just to be clear, we are not alone in our failings. Born in the euphoria of post-WWII American exceptionalism, I was raised to believe in the sanctity of our institutions and, indeed, in our innate decency as

Americans. Saying the Pledge of Allegiance every morning as a schoolboy, I stood before our flag and affirmed that the United States was a land marked by “liberty and justice for all.” On the schoolyard, however, I would hear children retell their parents’ jokes, jokes made at the expense of Italian and Polish people. Heading home to watch the evening news, I saw Governor George Wallace try to bar courageous students of color from entering the University of Alabama. So began my education in the ways of the world. A high school student in 1968, I witnessed our military, the envy of the world two decades earlier, murder hundreds of civilians in My Lai. Two years later, our soldiers turned their weapons on America’s citizens, killing four Kent State students who stood in protest of our war in Vietnam. In college, I watched our system of government, so admired the world over, nearly come undone by the spectacle of corruption in the Nixon administration.

Sadly, we find misconduct in every organization and institution. The 1919 Black Sox scandal and, more recently, Pete Rose’s gambling habits and so many athletes’ use of performance-enhancing drugs remind us that even that icon of American wholesomeness, baseball itself, can be undone by human failings. Business leaders, stewards of investments of all kinds and bound by fiduciary duties of care and loyalty, can still lead a firm like Enron into the ground. One of our most revered charities, the United Way, was rocked by scandal when its longtime CEO, William Aramony (who served from 1970 to 1992), was convicted on more than 20 counts of fraud, conspiracy, and the like. Incredibly, he diverted \$1.2 million of the charity’s money to pay for his affair with a teenage mistress (Arenson, 1995).

Recent killings of African Americans in places like Baltimore (Freddie Gray), Chicago (Laquan McDonald), Ferguson (Michael Brown), North Charleston (Walter Scott), and St. Paul (Philando Castile) tell us that our police forces, here to serve and protect, might be riven with racism. Finally, let’s consider those who wear the robes. We know that judges, those tasked with administering blind justice, can be bought. Cook County Judge Thomas Maloney’s 1993 conviction for taking bribes to fix murder cases tells us so (O’Connor, 1993). Holy people? Whether at the individual or the institutional level of analysis, we can be left disillusioned. For example, popular televangelists Jim Bakker and Jimmy Swaggart fell from grace in the 1980s and 1990s (for having sex with his secretary and fraudulently selling time shares in Bakker’s case, and for being caught with a prostitute on two separate occasions in Swaggart’s case), and

the Catholic Church’s countenance of child abuse for decades is repellant. Whom are we kidding when we academics imagine that we are free of failings? What to do?

To be sure, we need to recognize our problems and take steps to mitigate them. In fact, we have made good progress in this regard. We enshrined our professional ideals in the Academy of Management Code of Ethics and, beyond that, made it an enforceable code and not just an aspirational one. Aware of the allure of what Mone and McKinley (1993) called the “uniqueness value” in organizational studies (i.e., where our scholarship is most prized for being new and different), we created a new journal, *Academy of Management Discoveries*, that explicitly welcomes replication research. Hearing calls to amend our editorial practices to ensure the integrity of the research process (cf. Schwab & Starbuck, 2017; Starbuck, 2016), a number of journals are doing just that [cf. *Research Policy* (Martin, 2013), *Management and Organization Review* (Lewin et al., 2016), and *Strategic Management Journal* (Bettis et al., 2016)]. And yes, we encourage colleagues like Honig and Lampel to foster the reflection and conversation we see here. Note the many ideas for reform we read in these pages.

Asking us to “follow an engaged scholarship model to seek the meaning of research” and to move “from instrumental research to responsible science,” Runtian Jing and Anne Tsui, respectively, hope that a reaffirmation of our sacred aspirations will help us resist the temptation of scientific subterfuge. However, our understanding of moral licensing and moral hypocrisy tells us to beware such urgings. As Batson (2008, p. 51) observed, “The goal of moral hypocrisy is to appear moral yet, if possible, avoid the cost of being moral.” Unfortunately, the affirmation of engaged and responsible scholarship can help hypocrites keep up appearances. And so, we also hear calls for better policing. Jing asks us to establish a social control system where “researchers are encouraged to officially report academic misconduct around them.” David Sirmon wants to see our journals “publish and update questionable research practice documents.” Not mincing words, Mary Ann Glynn asks us to “dismantle the current pilings that buttress our institutions that publish scholarly research while simultaneously building ones that support and applaud the norms of appropriate and scholarly research.”

This focus on the journals is of a piece with ideas to reform our entire research ecosystem. Joel Baum, for example, wants us to better “coordinate the fragmented activities of deans, journal editors, granting

agencies, and academic committees” to, in Glynn’s words, make our review processes “more humane.” Looking even more broadly at national-level actors, Elke Schübler applauds Germany’s premier scholarly association for its effort to delegitimize current practices in the country, practices that she believes foster “a mechanistic academic career system.” Perhaps our problems will be solved once we affirm the better nature of our angels and reform our scholarly institutions. If only it were so. Sadly, as well intentioned, important, and even noble as these efforts are, they will fail if we assume that they are aimed at someone else, at the miscreants in our midst who so dishonor us.

Merton (1942/1973) celebrated the humility of scientific genius. To be sure, it helps to remember that we, like Isaac Newton, are a part of an intergenerational quest of discovery.<sup>10</sup> Our work does not begin and end with us. Beyond that, humility is also born of a recognition of fallibility. Each one of us is capable of misunderstanding, mistakes, and yes, even misconduct. We are never as ethical as we think we are (Tenbrunsel, Diekmann, Wade-Benzoni, & Bazerman, 2010). That said, our house is not on fire. I believe that the vast majority of scientists—and yes, politicians, military officers, baseball players, business and not-for-profit leaders, police officers, judges, and priests—are people of integrity. Still, some of us will inevitably stumble and fall. We ensure our future only when we each recognize that we might be the one to so fall. Humility really does matter. Without it, our hard-won self-confidence can turn to hubris in the blink of an eye.<sup>11</sup>

## RECOMMENDATIONS, PROPOSAL, AND POTENTIAL REMEDIES

### A Proposed Reviewer Code of Conduct

Researchers may commit misconduct to improve their chances of publishing, but research also suggests that researchers may also commit misconduct if they believe they have been unfairly treated by the peer-review system. Clair (2015, p. 159) argued that “procedural injustice in the peer-review system encourages scientific misconduct by generating

feelings of cynicism about the legitimacy of the peer-review system and by lowering scholars’ felt obligation to abide by accepted norms for scientific conduct.” Addressing procedural injustice is therefore not only an important goal in and of itself—since fairness in the interaction between authors and reviewers, as well as editors, is clearly desirable—it also reduces cynicism that may lead to misconduct.

In our opinion, there are two reforms that journals can undertake to reduce procedural injustice. First, it may be time for journals to develop a code of conduct for reviewers that goes beyond the usual injunction to be “constructive.” Most reviewers are competent and fair, but it is difficult to deny that some reviewers intentionally or unintentionally use their power unwisely. A code of conduct for reviewers should address practices that we believe often give rise to a sense of unfairness that results from the actions of a minority. The development of such a code is clearly beyond the scope of this paper—this is most likely a task for the Academy of Management, or a consortium of journals—but for purposes of illustration we can point to a number of issues that should be addressed. For example, a reviewer opining a strong objection to a particular theory, rather than the way the theory is used by the authors, is manifestly unfair if the authors’ work is in another theoretical tradition. A sense of unfairness will be compounded if the reviewer engages in “coercive reframing”—trying to force authors to adopt his or her preferred theory, often with the implicit threat of manuscript rejection if the suggestion is declined (see Shibayama & Baba, 2015). More generally, a code of conduct should discourage sweeping dismissal of an entire manuscript that is not based on specific details, as such sweeping dismissal is neither helpful nor fair to the authors.

Second, discouraging procedural injustice may not depend only on a code of conduct that provides reviewers with a clearer framework of their responsibilities; it also should involve giving authors more voice. Currently, most journals allow reviewers to confidentially communicate with the editor handling the manuscript. This private communication provides reviewers the opportunity to explain their assessment more fully or more directly, or put their decision in a wider context. A similar privilege should be accorded to authors. Regardless of the decision outcome, authors should be encouraged to provide assessments of the blind reviewer’s editorial process. To deter long complaints, the system should clearly ask authors to signal specific aspects of the

<sup>10</sup> Recall Newton’s famous words, “If I have seen further it is by standing on the shoulders of giants” (Merton, 1942/1973, p. 273).

<sup>11</sup> Acknowledgment: I would like to thank Sue Ashford, Joshua Margolis, Lance Sandelands, and the reviewers for their comments on an earlier version of this essay.

reviews that cross the line from fair to unfair reviewing. In effect, such a system would be the mirror image of the code of conduct for reviewers. If a reviewer is pressuring authors to HARK (i.e., change hypotheses in light of results), authors can point this issue out to the editor, rather than find themselves in the awkward position that Joel Baum (see above) found himself when he had to point out to the reviewer that such an act is considered a violation of research ethics.

Implementing a code of conduct for reviewers, and giving authors more voice, creates what amounts to a 360-degree peer review process. Reviewers communicate privately with editors while aware that authors can flag review issues with editors. These steps should improve the perception of fairness in the peer review process, and thus, in our opinion, at least curtail the sense of many authors that when it comes to publishing, the deck is stacked against them. It is also worthwhile to consider whether a 360-degree peer review process should also include the editors handling the manuscript. Most of what we know about the process of appointing and promoting editors is anecdotal, based on our personal experience. It is unclear whether systematic feedback to chief editors and editorial boards is part of the process. The opportunity for authors and reviewers to deposit feedback about their journal experience may be useful for making such decisions, and will also be an important source of information for improving the journal more generally. Such transparency ratings, and fairness ratings, may sit with journals alongside other sources of data, such as impact factor, to inform potential authors about the suitability of a particular outlet (Corley & Schinoff, 2017).

### **Replication: Validating the Quality of Management Scholarship**

The validity of contemporary research is not only challenged by the forces of hypercompetition, but is exacerbated by the prevalence of marketing scholarship through avenues such as TED Talks, consulting, and the sales of nonacademic books (Honig, Lampel, Siegel, & Drnevich, 2017). Distinguishing between accurate and reliable scholarship is increasingly important in a world of “fake news” and virtual media hype. Replication is increasingly an essential if overlooked tool to validate management scholarship and bridge the gap between rigor and relevance (Gulati, 2007). Encouraging replication requires a multifaceted approach. First, leading journals must begin to recognize the value, and begin

dedicating a percentage of their publication space to replication studies. Second, journals must make greater demands on the part of authors to facilitate future replication. This would include insisting on the submission of replication “road maps” that would provide future scholars with the specific details of methods utilized, including scripts, data coding details, time and effort to acquire data, and anonymized data sets that ensure proprietary rights and anonymity while offering the opportunity for others to independently validate results. Reifying processes that expedite replication requires a cultural shift in management scholarship, but one we believe will be an essential component of increasing both quality and relevance.

### **Authors’ Assessment of Collective Recommendations**

The essays by our nine scholars contain not only analysis and reflections, but also recommendations for how our field can reduce ethical lapses. These recommendations range from systemic reforms and institutional change (e.g., implementing new procedures for tenure review committees that deemphasize impact factors, instituting codes of conduct) to the technical and procedural (e.g., new methods of evaluating statistics, replication, and control systems; new reviewing procedures). In all, we identified 13 significant proposals that we felt should be listed separately and evaluated by all the authors for their desirability and potential effectiveness. The recommendations and the evaluations are presented in Table 1.

As can be seen from the data, there was nearly universal agreement on the recommendation that tenure committees should engage in deep assessment rather than relying on quantification of performance (e.g., counting the number of publications, assessing the impact factor of the journals in which they appeared, and recording their citation rates). To some extent, this represents a return to an earlier era, before the easy availability of impact and citation data, when promotion and tenure committees were inclined by necessity (and perhaps also by tradition) to evaluate their colleagues’ performance in depth. In today’s environment where the pressures on academic professionals are greater than ever and where quantitative data are easily available, adopting more in-depth assessment may require significant cultural change. Perhaps a recommendation from the Academy of Management or an accreditation board such as the Association to Advance Collegiate Schools of



TABLE 1  
Evaluation of Responses to Ethical Compromises for Management Research, Ranked and Evaluated by Authors

	Ranked as 1 or 2 by at least 2 authors	Completely ineffective (1)	Somewhat ineffective (2)	Neither ineffective nor effective (3)	Somewhat effective (4)	Very effective (5)	Responses	Weighted average
Tenure recommendations based on deep assessment rather than numbers	X	1	0	0	3	6	10	4.3
Code of conduct for reviewers	X	0	0	3	5	2	10	3.9
Dynamic and open publishing practices		0	2	1	4	3	10	3.8
Formal QRP policies for journals		1	0	1	6	2	10	3.8
Dedicated journal space for replication	X	2	0	0	5	3	10	3.7
Special reviewers tasked with examining methodology		1	1	1	4	3	10	3.7
Better policies for statistical significance		1	0	2	5	2	10	3.7
Required road maps of methods used to facilitate replication		2	0	0	5	3	10	3.7
Scientific Wikipedia	X	1	2	2	4	2	10	3.7
Social control system		1	1	3	4	1	10	3.5
Indigenous research assessment		1	1	4	3	1	10	3.0
Engaged scholarship model		1	1	3	5	0	10	3.2
360-degree reviews	X	1	2	3	3	1	10	3.1

Business (AACSB) would be helpful in instituting such a change.

Many of the recommendations addressed changes in publishing practices. Developing a code of conduct for reviewers was both highly ranked and highly rated. The Committee on Publication Ethics (COPE), as well as the Academy, could be helpful in instituting such a code. Close behind was support for providing dedicated space in journals for replication. This may well reflect the reality confronting researchers who contemplate doing replication research: There currently exist very few avenues for such research. Providing such opportunities should serve to validate and/or challenge much of our scholarship, an important step to ensuring quality and ethical compliance. Even if such opportunities emerge, another obstacle that confronts replication is the lack of clarity when research design is outlined. What would also help, agree our authors, is requiring authors to provide a road map for replication that will not only make it easier to replicate a study, but will also provide common ground for comparing the results of different replication efforts.

Worth noting are recommendations that urge open publishing, more explicit policies that alert authors to questionable research practices, better policies for dealing with the contentious issues that accompany the use of statistical significance, and special reviewers to vet the sophisticated statistical methods that are increasingly being used today. The radical idea of engaging in 360-degree assessment by authors of reviewers and editors was the least supported journal reform. Although one can speculate about whether this is because the idea is regarded as impractical or politically untenable, it is hard to avoid the need for greater accountability for gatekeepers. More popular were recommendations for creating shared resources such as a scientific Wikipedia and greater acceptance for research that engages with pressing social, political, and ecological issues that face the world today. Overall, the statistical weighted averages for most of the recommendations didn't demonstrate a huge variance, suggesting that many of the ideas presented in these essays warrant further consideration and development.

## In Parting

While many of us are aware of increasing constraints and share a growing concern regarding the ethical direction of our scholarly community, scholarly debate and dialogue has, until recently, been rather limited. In this study, we took the unusual

approach of asking members of our community—some prominent and at the height of their careers, others just starting their scholarly journeys—to provide personal opinions on what many regard as a crisis. Our respondents, perhaps mirroring their own academic specialties, often chose to orient themselves toward either a macro or a micro level of analysis. Irrespective of which level of analysis was adopted, most contributors recognized and identified numerous conditions that led to our current situation. They frequently cited systemic and institutional norms, arguing that editors, deans, reviewers, and authors all deserve an equal share of responsibility for the current dilemmas we face. Solutions were mixed. Some advocated specific processes and techniques, others called for a systemic revolution, and one author (James Walsh) simply pointed out that we should not expect scholars to be different from any other public servants, foibles included. This is an important argument often overlooked when examining the impact of ethics on scholarship. While we may think of our academic mission as inhabiting a stratum above that of other human endeavors, our collective behavior may, in fact, mirror more modest, fallible, and even self-interested behavior more reflective of arenas recognized as highly competitive. Thus, our sense of idealized competition and collaboration for the greater public good may, in fact, be undermined by basic universal human limitations.

We hope that this project represents a new beginning, whereby we publicly and collegially address, debate, and discuss many of the antecedents that explain our current research and ethical dilemmas, as well as alternative avenues to ameliorate ethical lapses and to improve our scholarly relevance and capabilities. We look forward to continuing this dialogue in various ways throughout our community. To that end, the editors have provided us with a community blog space to further this dialogue with our colleagues. The follow-up blog associated with this article will be at <https://ethicist.aom.org> and will be under the heading Reflections on Scientific Misconduct in Management: Discussion Space. We very much look forward to continuing to explore and expand this important conversation with our colleagues, and we encourage your participation.

## REFERENCES

- Adler, N. J., & Harzing, A.-W. (2009). When knowledge wins: Transcending the sense and nonsense of academic rankings. *Academy of Management Learning & Education*, 8(1), 72–95.

- Aguinis, H. A., Shapiro, D. L., Antonacopoulou, E. P., & Cummings, T. G. (2014). Scholarly impact: A pluralist conceptualization. *Academy of Management Learning & Education*, 13(4), 623–639.
- Alvesson, M., & Sandberg, J. (2013). Has management studies lost its way? Ideas for more imaginative and innovative research. *Journal of Management*, 50(1), 128–152.
- Arenson, K. W. (1995, May 23). Ex-United Way leader gets 7 years for embezzlement. *New York Times*. Retrieved from <http://www.nytimes.com/1995/06/23/us/ex-united-way-leader-gets-7-years-for-embezzlement.html>
- Banks, G. C., et al. (2016). Questions about questionable research practices in the field of management: A guest commentary. *Journal of Management*, 42(1), 5–20.
- Barkema, H. G., Chen, X. P., George, G., Luo, Y., & Tsui, A. S. (2015). West meets East: New concepts and theories. *Academy of Management Journal*, 58(2), 460–479.
- Barley, S. R. (2016). 60th anniversary essay: Ruminations on how we became a mystery house and how we might get out. *Administrative Science Quarterly*, 61, 1–8.
- Batson, C. D. (2008). Moral masquerades: Experimental exploration of the nature of moral motivation. *Phenomenology and the Cognitive Sciences*, 7(1), 51–66.
- Batson, C. D., Thompson, E. R., & Chen, H. (2002). Moral hypocrisy: Addressing some alternatives. *Journal of Personality and Social Psychology*, 83(2), 330–339.
- Baum, J. A. C. (2011). Free-riding on power laws: Questioning the validity of the impact factor as a measure of research quality in organization studies. *Organization*, 18, 449–466.
- Baum, J. A. C., & Bromiley, P. (2017). *P-hacking: Scholarly dishonesty in response to publication pressures* (Working paper). University of Toronto/University of California, Irvine.
- Bedeian, A. G., Taylor, S. G., & Miller, A. N. (2010). Management science on the credibility bubble: Cardinal sins and various misdemeanors. *Academy of Management Learning & Education*, 9(4), 715–725.
- Bergh, D. D., Sharp, B. M., & Li, M. (2017). Tests for identifying “red flags” in empirical findings: Demonstration and recommendations for authors, reviewers, and editors. *Academy of Management Learning & Education*, 16(1), 110–124.
- Berman, E. P. (2012). *Creating the market university: How academic science became an economic engine*. Princeton, NJ: Princeton University Press.
- Bettis, R. A. (2012). The search for asterisks: Compromised statistical tests and flawed theories. *Strategic Management Journal*, 33, 108–113.
- Bettis, R. A., Ethiraj, S., Gambardella, A., Helfat, C., & Mitchell, W. (2016). Creating repeatable cumulative knowledge in strategic management. *Strategic Management Journal*, 37(1), 257–261.
- Birkinshaw, J., Healey, M. P., Suddaby, R., & Weber, K. (2014). Debating the future of management research. *Journal of Management Studies*, 51, 38–55.
- Blanken, I., Van de Ven, N., & Zeelenberg, M. (2015). A meta-analytic review of moral licensing. *Personality and Social Psychology Bulletin*, 41(4), 540–558.
- Bohannon, J. (2016). Hate journal impact factors? New study gives you one more reason. *Science*. doi: 10.1126/science.aag0643truejrn
- Bok, D. (2009). *Beyond the ivory tower: Social responsibilities of the modern university*. Cambridge, MA: Harvard University Press.
- Brumback, R. A. (2008). Worshiping false idols: The impact factor dilemma. *Journal of Child Neurology*, 23, 365–367.
- Butler, N., & Spoelstra, S. (2012). Your excellency. *Organization*, 19, 891–903.
- Certo, S. T., Sirmon, D. G., & Brymer, R. (2010). Competition and knowledge creation in management: Investigating changes in scholarship from 1988 to 2007. *Academy of Management Learning & Education*, 9(4), 591–606.
- Charness, G., Masclet, D., & Villeval, M. C. (2014). The dark side of competition for status. *Management Science*, 60, 38–55.
- Churchman, C. W. (1948). Statistics, pragmatics, induction. *Philosophy of Science*, 15, 249–268.
- Clair, J. A. (2015). Procedural injustice in the system of peer review and scientific misconduct. *Academy of Management Learning & Education*, 14, 159–172.
- Community for Responsible Research in Business and Management. (2017). *A vision of responsible research in business and management: Striving for credible and useful knowledge* (White paper). Retrieved from [https://rrbm.network/wp-content/uploads/2017/09/White\\_Paper.pdf](https://rrbm.network/wp-content/uploads/2017/09/White_Paper.pdf)
- ConsultingNewsLine. (2008). Interview with Diane Vaughan. Retrieved from <http://www.consultingnewsline.com/Info/Vie%20du%20Conseil/Le%20Consultant%20du%20mois/Diane%20Vaughan%20%28English%29.html>
- Corbett, A., Cornelissen, J., Delios, A., & Harley, B. (2014). Variety, novelty and perceptions of scholarship in research on management and organizations: An appeal for ambidextrous scholarship. *Journal of Management Studies*, 51, 3–18.
- Corley, K. G., & Schinoff, B. S. (2017). Who, me? An inductive study of novice experts in the context of how editors come to understand theoretical contribution. *Academy of Management Perspectives*, 31(1), 4–27.
- Davis, G. F. (2009). *Managed by the markets: How finance re-shaped America*. Oxford, UK: Oxford University Press.

- Davis, G. F. (2015). Editorial essay: What is organizational research for? *Administrative Science Quarterly*, 60(2), 179–188.
- Deci, E. L., & Ryan, R. M. (1985). *Intrinsic motivation and self-determination in human behavior*. New York: Plenum.
- De Rond, M., & Miller, A. N. (2005). Publish or perish: Bane or boon of academic life? *Journal of Management Inquiry*, 14, 321–329.
- De Vries, R., Anderson, M., & Martinson, B. (2006). Normal misbehavior: Scientists talk about the ethics of research. *Journal of Empirical Research on Human Research Ethics*, 1, 43–50.
- Douglas, H. (2009). *Science, policy, and the value-free ideal*. Pittsburgh, PA: University of Pittsburgh Press.
- Economist. (2013, September 28). Looks good on paper. *The Economist*. Retrieved from <https://www.google.com/search?client=safari&rls=en&q=Economist.+{2013,+September+28}.+Looks+good+on+paper&ie=UTF-8&oe=UTF-8>
- Espeland, W., & Stevens, M. L. (1998). Commensuration as a social process. *Annual Review of Sociology*, 24, 313–343.
- Fandos, N. P., & Pisner, N. B. (2013, April 11). Joining the ranks: Demystifying Harvard's tenure system. *Harvard Crimson*. Retrieved from <http://www.thecrimson.com/article/2013/4/11/scrutiny-tenure-harvard>
- Fanelli, D. (2010a). "Positive" results increase down the hierarchy of the sciences. *PLoS One*. doi: 10.1371/journal.pone.0010068.
- Fanelli, D. (2010b). Do pressures to publish increase scientists' bias? An empirical support from US states data. *PLoS One*. doi: 10.1371/journal.pone.0010271.
- Fanelli, D. (2012). Negative results are disappearing from most disciplines and countries. *Scientometrics*, 90, 891–904.
- Fanelli, D., Costas, R., & Larivière, V. (2015). Misconduct policies, academic culture and career stage, not gender or pressures to publish, affect scientific integrity. *PLoS One*. doi: 10.1371/journal.pone.0127556.
- Fang, F. C., Steen, R. G., & Casadevall, A. (2012). Misconduct accounts for the majority of retracted scientific publications. *Proceedings of the National Academy of Sciences of the United States of America*, 109(42), 17028–17033.
- Garfield, E. (2006). The history and meaning of the journal impact factor. *Journal of the American Medical Association*, 295, 90–93.
- Ghoshal, S. (2005). Bad management theories are destroying good management practices. *Academy of Management Learning & Education*, 4(1), 75–91.
- Gleick, J. (1987). *Chaos: Making a new science*. New York: Vintage.
- Goldfarb, B., & King, A. A. (2016). Scientific apophenia in strategic management research: Scientific tests and mistaken inference. *Strategic Management Journal*, 37(1), 167–176.
- Greenwood, R. (2016). OMT, then and now. *Journal of Management Inquiry*, 25(1), 27–33.
- Greve, H. R. (1998). Performance, aspirations, and risky organizational change. *Administrative Science Quarterly*, 43, 58–86.
- Gulati, R. (2007). Tent poles, tribalism, and boundary spanning: The rigor-relevance debate in management research. *Academy of Management Journal*, 50(4), 775–782.
- Hannan, M. T., & Freeman, J. (1977). The population ecology of organizations. *American Journal of Sociology*, 82, 929–964.
- Hayward, J. (1976). Institutional inertia and political impetus in France and Britain. *European Journal of Political Research*, 4, 341–359.
- Head, M. L., Holman, L., Lanfear, R., Kahn, A. T., & Jennions, M. D. (2015). The extent and consequences of p-hacking in science. *PLoS Biology*. doi: 10.1371/journal.pbio.1002106.
- Hempel, C. (1965). *Aspects of scientific explanation*. New York: Free Press.
- Honig, B., & Bedi, A. (2012). The fox in the hen house: A critical examination of plagiarism among members of the Academy of Management. *Academy of Management Learning & Education*, 11(1), 101–123.
- Honig, B., Lampel, J., Siegel, D., & Drnevich, P. (2014). Ethics in the production and dissemination of management research: Institutional failure or individual fallibility? *Journal of Management Studies*, 51, 118–142.
- Honig, B., Lampel, J., Siegel, D., & Drnevich, P. (2017). Special section on ethics in management research: Norms, identity, and community in the 21st century. *Academy of Management Learning & Education*, 16(1), 84–93.
- Hubbard, R., & Vetter, D. E. (1996). An empirical comparison of published replication research in accounting, economics, finance, management, and marketing. *Journal of Business Research*, 35, 153–164.
- Hull, D. L. (1988). *Science as a process*. Chicago: University of Chicago Press.
- Hussain, S. (2015). Journal list fetishism and the "sign of 4" in the ABS guide: A question of trust? *Organization*, 22, 119–138.
- Hvistendahl, M. (2015). China pursues fraudsters in science publishing. *Science*, 350(6264), 1015.
- Ioannidis, J. P. A. (2005). Why most published research findings are false. *PLoS Medicine*, 2, e124.

- Isaacson, W. (2008). *Einstein: His life and universe*. New York: Simon and Schuster.
- John, L. K., Loewenstein, G., & Prelec, D. (2012). Measuring the prevalence of questionable research practices with incentives for truth telling. *Psychological Science*, 23, 524–532.
- John, L. K., Loewenstein, G., & Rick, S. I. (2014). Cheating more for less: Upward social comparisons motivate the poorly compensated to cheat. *Organizational Behavior and Human Decision Processes*, 123, 101–109.
- Kaplan, A. (1964). *The conduct of inquiry: Methodology for behavioral science*. San Francisco: Chandler Publishing Company.
- Karabag, S. F., & Berggren, C. (2012). Retraction, dishonesty and plagiarism: Analysis of a crucial issue for academic publishing, and the inadequate responses from leading journals in economics and management disciplines. *Journal of Applied Economics and Business Research*, 2(4), 172–183.
- Keith-Spiegel, P., Sieber, J., & Koocher, G. P. (2010). *Responding to research wrongdoing: A user-friendly guide*. Boston: Simmons College. Retrieved from <https://research.uark.edu/documents/rsdp/responding-to-research-wrongdoing.pdf>
- Kepes, S., Bennett, A. A., & McDaniel, M. A. (2014). Evidence-based management and the trustworthiness of our cumulative scientific knowledge: Implications for teaching, research, and practice. *Academy of Management Learning & Education*, 13(3), 446–466.
- Kepes, S., & McDaniel, M. A. (2013). How trustworthy is the scientific literature in industrial and organizational psychology? *Industrial and Organizational Psychology: Perspectives on Science and Practice*, 6, 252–268.
- Kerr, N. L. (1998). HARKing: Hypothesizing after the results are known. *Personality and Social Psychology Review*, 2, 196–217.
- Khurana, R. (2010). *From higher aims to hired hands: The social transformation of American business schools and the unfulfilled promise of management as a profession*. Princeton, NJ: Princeton University Press.
- Kieser, A. (2012). JOURQUAL—der Gebrauch, nicht der Missbrauch, ist das Problem. Oder: Warum Wirtschaftsinformatik die beste deutschsprachige betriebswirtschaftliche Zeitschrift ist. *Die Betriebswirtschaft*, 72, 93–110.
- Kuhn, T. S. (1996). *The structure of scientific revolutions* (3rd ed.). Chicago: University of Chicago Press.
- Lacey, H. (1999). *Is science value free? Values and scientific understanding*. New York: Routledge.
- Levi, I. (1960). Must the scientist make value judgments? *Journal of Philosophy*, 57, 345–357.
- Lewin, A. Y., et al. (2016). The critique of empirical social science: New policies at management and organization review. *Management and Organization Review*, 12(4), 649–658.
- Lewis, B. R., Duchac, J. E., & Beets, S. D. (2011). An academic publisher's response to plagiarism. *Journal of Business Ethics*, 102, 489–506.
- Macdonald, S. (2015). Emperor's new clothes: The reinvention of peer review as myth. *Journal of Management Inquiry*, 24, 264–279.
- Martin, B. (2013). Whither research integrity? Plagiarism, self-plagiarism and coercive citation in an age of research assessment. *Research Policy*, 42, 1005–1014.
- Martinson, B. C., Anderson, M. S., & de Vries, R. (2005). Scientists behaving badly. *Nature*, 435, 737–738.
- Mažar, N., Amir, O., & Ariely, D. (2008). The dishonesty of honest people: A theory of self-concept maintenance. *Journal of Marketing Research*, 45, 633–644.
- Mažar, N., & Ariely, D. (2012). "What the hell": Continuous temptations and escalation of dishonesty (Working Paper). Toronto: University of Toronto, Rotman School of Management.
- McMullin, E. (1982). Values in science. In *PSA: Proceedings of the biennial meeting of the Philosophy of Science Association*, 4, 3–28. doi: 10.1111/j.1467-9744.2012.01298
- Melander, H., Ahlqvist-Rastad, J., Meijer, G., & Beermann, B. (2003). Evidence b(i)ased medicine—Selective reporting from studies sponsored by pharmaceutical industry: Review of studies in new drug applications. *British Medical Journal*, 326, 1171–1173.
- Merritt, A. C., Effron, D. A., & Monin, B. (2010). Moral self-licensing: When being good frees us to be bad. *Social and Personality Psychology Compass*, 4/5, 344–357.
- Merton, R. (1942/1973). The normative structure of science. In R. Merton (Ed.), *The sociology of science: Theoretical and empirical investigations* (pp. 267–280). Chicago, IL: University of Chicago Press.
- Merton, R. K. (1957). Priorities in scientific discovery: A chapter in the sociology of science. *American Sociological Review*, 22, 635–659.
- Meyer, K. E., van Witteloostuijn, A., & Beugelsdijk, S. (2017). What's in a p? Reassessing best practices for conducting and reporting hypothesis-testing research. *Journal of International Business Studies*, 48(5), 535–551.
- Mezias, S. J., & Regnier, M. O. (2007). Walking the walk as well as talking the talk: Replication and the normal science paradigm in strategic management research. *Strategic Organization*, 5, 283–296.
- Miller, A. N., Taylor, S. G., & Bedeian, A. G. (2011). Publish or perish: Academic life as management faculty live it. *Career Development International*, 16(5), 422–445.

- Mone, M. A., & McKinley, W. (1993). The uniqueness value and its consequences for organization studies. *Journal of Management Inquiry*, 2, 284–296.
- Mulkay, M. (1969). Some aspects of cultural growth in science. *Social Research*, 36, 22–52.
- Necker, S. (2014). Scientific misbehavior in economics. *Research Policy*, 43, 1747–1759.
- O'Boyle, E. H., Jr., Banks, G. C., & Gonzalez-Mulé, E. (2014). The chrysalis effect: How ugly initial results metamorphosize into beautiful articles. *Journal of Management*, 43, 376–399.
- O'Connor, M. (1993, April 17). Judge Maloney found guilty in corruption case. *Chicago Tribune*. Retrieved from [http://articles.chicagotribune.com/1993-04-17/news/9304170108\\_1\\_judge-maloney-bribes-convicted](http://articles.chicagotribune.com/1993-04-17/news/9304170108_1_judge-maloney-bribes-convicted)
- Oliver, C. (2010). The goals of scholarship. *Journal of Management Inquiry*, 19, 26–32.
- Olson, M. (1965). *The logic of collective action*. Cambridge, MA: Harvard University Press.
- Open Science Collaboration. (2015). Estimating the reproducibility of psychological science. *Science*, 349(6251), aac4716.
- Parker, M., & Weik, E. (2014). Free spirits? The academic on the aeroplane. *Management Learning*, 45(2), 167–181.
- Pearce, J. L., & Huang, L. (2012). The decreasing value of our research to management education. *Academy of Management Learning & Education*, 11, 247–262.
- Proctor, R. N. (1991). *Value-free science: Purity and power in modern knowledge*. Cambridge, UK: Cambridge University Press.
- Quack, S., & Schüßler, E. (2015). Dynamics of regulation of professional service firms: National and transnational developments. In L. Empson, D. Muzio, & J. Broschak (Eds.), *The Oxford handbook of professional service firms* (pp. 48–70). New York: Oxford University Press.
- Reedijk, J., & Moed, H. F. (2008). Is the impact of journal impact factors decreasing? *Journal of Documentation*, 64, 183–192.
- Reichenbach, H. (1951). *The rise of scientific philosophy*. Berkeley, CA: University of California Press.
- Risjord, M. (2014). *Philosophy of social science: A contemporary introduction*. New York: Routledge.
- Rudner, R. (1953). The scientist qua scientist makes value judgments. *Philosophy of Science*, 20, 1–6.
- SAGE. (2014). SAGE statement on *Journal of Vibration and Control*. Retrieved from <https://us.sagepub.com/en-us/nam/press/sage-statement-on-journal-of-vibration-and-control>
- Schwab, A., & Starbuck, W. H. (2017). A call for openness in research reporting: How to turn covert practices into helpful tools. *Academy of Management Learning & Education*, 16, 125–141.
- Shibayama, S., & Baba, Y. (2015). Dishonest conformity in peer review. *Prometheus*, 33(3), 215–233.
- Simmons, J. P., Nelson, L. D., & Simonsohn, U. (2011). False-positive psychology: Undisclosed flexibility in data collection and analysis allows presenting anything as significant. *Psychological Science*, 22, 1359–1366.
- Spadero, A. (2013). A big heart open to God. *America*. Retrieved from <http://americamagazine.org/pope-interview>
- Starbuck, W. H. (2005). How much better are the most prestigious journals? The statistics of academic publication. *Organization Science*, 16, 180–200.
- Starbuck, W. H. (2016). 60th anniversary essay: How journals could improve research practices in social science. *Administrative Science Quarterly*, 61, 165–183.
- Starkey, K. (2015). The strange absence of management during the current financial crisis. *Academy of Management Review*, 40, 652–663.
- Swidler, A. (1986). Culture in action: Symbols and strategies. *American Sociological Review*, 51(2), 273–286.
- Tenbrunsel, A. E., Diekmann, K. A., Wade-Benzoni, K. A., & Bazerman, M. H. (2010). The ethical mirage: A temporal explanation as to why we are not as ethical as we think we are. *Research in Organizational Behavior*, 30, 153–173.
- Thornton, P. H., Ocasio, W., & Lounsbury, M. (2012). *The institutional logics perspective: A new approach to culture, structure and process*. Oxford, UK: Oxford University Press.
- Tourish, D. (2011). Leading questions: Journal rankings, academic freedom and performativity: What is, or should be, the future of leadership? *Leadership*, 7, 367–381.
- Tsui, A. S. (2009). Autonomy of inquiry: Shaping the future of emerging scientific communities. *Management and Organization Review*, 5(1), 1–14.
- Tsui, A. S. (2013). The spirit of science and socially responsible scholarship. *Management and Organization Review*, 9(3), 375–394.
- Tsui, A. S. (2016). Reflections on the so-called value-free ideal: A call for responsible science in the business schools. *Cross Cultural and Strategic Management Journal*, 23(1), 4–28.
- Van de Ven, A. H. (2007). *Engaged scholarship: A guide for organizational and social research*. Oxford, UK: Oxford University Press.
- Van de Ven, A. H., & Jing, R. (2012). Indigenous management research in China from an engaged scholarship perspective. *Management and Organization Review*, 8(1), 123–137.

- van Witteloostuijn, A. (2016). What happened to Popperian falsification? Publishing neutral and negative findings: Moving away from biased publication practices. *Cross Cultural & Strategic Management*, 23(3), 481–508.
- Vaughan, D. (1997). *The Challenger launch decision: Risky technology, culture, and deviance at NASA*. Chicago: University of Chicago Press.
- Walsh, J. P. (2011). Embracing the sacred in our secular scholarly world. *Academy of Management Review*, 36(2), 215–234.
- Weick, K. E. (1995). *Sensemaking in organizations*. Thousand Oaks, CA: SAGE.
- Wilhite, A. W., & Fong, E. A. (2012). Coercive citation in academic publishing. *Science*, 335, 542–543.
- Wolpe, P. R. (2006). Reasons scientists avoid thinking about ethics. *Cell*, 125(6), 1023–1025.
- Yang, D. (2016). A-journal publications of five mainland Chinese universities in management areas. *Internal Report of Antai College of Economics and Management of Shanghai Jiaotong University* (in Chinese).
- Zeng, W., & Resnik, D. (2010). Research integrity in China: Problems and prospects. *Developing World Bioethics*, 10(3), 164–171.
- Zhong, C. B. (2011). The ethical dangers of deliberative decision making. *Administrative Science Quarterly*, 56(1), 1–25.
- Zuckerman, H. (1977). Deviant behavior and social control in science. In E. Sagarin (Ed.), *Deviance and social change* (pp. 87–138). Beverly Hills, CA: SAGE.



**Joel A. C. Baum** (jbaum@rotman.utoronto.ca) is George E. Connell Chair in Organizations and Society at the University of Toronto. His research emphasizes patterns of competition and cooperation among firms and their influence on firm behavior and learning. His interest in academic performance measurement developed over eight years as associate dean of faculty.

**Mary Ann Glynn** (maryann.glynn@bc.edu) is the Joseph F. Cotter Professor of Management and Organization at Boston College. She earned her Ph.D. at Columbia University and focuses her research on combining micro-level cognitive processes and macro-level cultural influences (social norms and institutional arrangements), with both qualitative and quantitative methods.

**Benson Honig** (bhonig@mcmaster.ca) is the Teresa Cascioli Chair in Entrepreneurial Leadership at the DeGroote School of Business at McMaster University in Hamilton, Ontario. Studying entrepreneurship worldwide, his research includes business planning, nascent entrepreneurship,

transnational entrepreneurship, ethics in scholarship, social entrepreneurship, social capital, and environments of transition. He has published widely, with more than 80 articles and books.

**Runtian Jing** (rtjing@sjtu.edu.cn) is a professor of organizational behavior at Shanghai Jiaotong University. His research interests include organizational change and leadership behavior. He currently serves as the vice president of the International Association for Chinese Management Research. He received his Ph.D. from Xi'an Jiaotong University in 1997.

**Joseph Lampel** (joseph.lampel@manchester.ac.uk) is Eddie Davies Professor of Enterprise and Innovation Management at Alliance Manchester Business School at the University of Manchester in England. He earned his Ph.D. at McGill University, and his main areas of research are strategic management, competitive dynamics, technological innovation, the interaction of strategy and governance, organizing in creative industries, and strategy in project-based industries.

**Michael Lounsbury** (ml37@ualberta.ca) is the Canada Research Chair in Entrepreneurship and Innovation at the University of Alberta School of Business. He is the series editor of *Research in the Sociology of Organizations* and has served as chair of the Academy of Management's OMT Division. His Ph.D. is from Northwestern University.

**Elke Schüßler** (elke.schuessler@jku.at) is a professor of business administration at Johannes Kepler University in Linz, Austria. Her recent research examines the governance of labor and environmental standards in global production networks, processes of organizing creativity in communities and networks, and dynamics of change in organizational fields, with a recent focus on digital markets.

**David G. Sirmon** (dsirmon@uw.edu) is an associate professor and the Robert Herbold Professor in Entrepreneurship at the Foster School of Business at the University of Washington in Seattle. He received his Ph.D. from the W. P. Carey School of Business at Arizona State University. His research focuses on resource orchestration, firm governance, family business, and entrepreneurship.

**Anne S. Tsui** (atsui@nd.edu) is a distinguished adjunct professor of management at the University of Notre Dame, the 67th president of AOM, the 14th editor of *AMJ*, founding president of IACMR, founding president of IACMR, and founding editor of *MOR*. She is also a co-founder of the Community for Responsible Research in Business and Management ([www.rrbm.network](http://www.rrbm.network)).

**Arjen van Witteloostuijn** (a.van.witteloostuijn@vu.nl) is a professor of business and economics at VU Amsterdam in the Netherlands, where he is dean of the School of Economics and Business, and the University of Antwerp in Belgium. He has published about 180 articles in international journals, including the *Academy of*

*Management Journal*, the *Academy of Management Review*, *Organization Science*, and *Strategic Management Journal*.

**Jim Walsh** (jpw Walsh@umich.edu) is a longtime professor at the University of Michigan's Ross School of Business. He explores how business serves, and should serve, society. Engaging his students with these questions, he helps prepare the next generation to lead. Jim served as the 65th president of the Academy of Management.

---

## APPENDIX

### Letter of invitation:

#### Dear Scholar,

A number of recent highly publicized incidents of scientific misconduct have led a number of scholars to argue that we are experiencing a crisis of research integrity in the social sciences in general and management research in particular. To get the conversation going, we include in this letter of invitation our thoughts regarding how we view this crisis.

Concerns about scientific integrity are arguably as old as science itself. Certainly, one can see these concerns in writings that seek to codify the scientific method as far back as the 17th century. For us, however, a useful starting point for examining threats to research integrity is the highly influential essay penned in 1942 by Robert Merton. In "The Normative Structure of Science" Merton sought to defend the autonomy of scientific research against intrusion and interference by Fascist and Stalinist regimes. Merton emphasized the uniqueness of science by pointing out that research did not rely on external enforcement of conduct by courts or government administrators, but on a set of interlocking norms and values that fashioned a "scientific conscience": an ethos that is not codified, but can be inferred from the "moral consensus of scientists as expressed in use and wont." This ethos, in Merton's view, was the main contributing factor for the "virtual absence of fraud in the annals of science," but so was the fact that "scientists are recruited from the ranks of those who exhibit an unusual degree of moral integrity."

A string of widely publicized research misconduct episodes in the social sciences, including management research, pose a challenge to Merton's argument. Not surprisingly, the first reaction to these cases has been to dismiss them as violations of norms whose identification attests to the strengths of the scientific research system. However, as researchers seek more systematic evidence, there is increasing recognition that contrary to Merton, errors and possible misconduct may be more commonplace in the social sciences than previously assumed (Open Science Collaboration, 2015; Karabag & Berggren,

2012). As the number of cases of misconduct and the number of retractions proliferate, the question that is increasingly being asked is whether the institutional evolution of management research has created conditions that encourage rather than discourage misconduct.

We can generally agree that scientific research has changed since Merton penned his essay. The scale of research today in terms of magnitude, scope, and cultural and geographic reach is so much greater than it was in 1942 that we cannot assume that the communal and normative forces that kept misconduct in check still apply. We no longer confront the specter of coercion from Fascism or Stalinism, but arguably we face a threat from the rise of entrepreneurialism: the expansion of a research culture that in many ways mirrors the best and worst of entrepreneurial activity in the wider economy. On the positive side, entrepreneurialism entails vigorous efforts to innovate and break new ground in research, to challenge convention and look for imaginative methods. On the negative side, it may lead to attempts to establish first-mover advantage at the expense of cooperation and acknowledgment of commonality with other research streams, and it promotes secrecy, encourages methodological corner cutting, and increasingly leads to tacit acceptance of misconduct as the necessary means to valuable ends.

Merton was not unaware of the problem of competition in scientific research, but he assumed that peer pressure and the sanctions imposed by the community would discourage deviance. As he stated, "The activities of scientists are subject to rigorous policing, to a degree perhaps unparalleled in any other field of activity." What he did not reckon with was competition at multiple levels: not only competition among researchers for credit and prestige, but competition among journals for citation impact, competition among university departments for prestige, and competition among universities for funding. Researchers today face far greater pressures with attendant greater rewards to produce research that is innovative and citation worthy. The pressures come from their departments, which are now engaged in corporate type annual reviews of performance, and from journals that explicitly discriminate against replication, and from submissions that are judged by their citation potential. Management research that used to be confined almost entirely to North America and Europe is rapidly becoming global. As the pool of active researchers expands, competition for publication space is exacerbated by the dominance of a small and highly selective group of elite A+ journals. We may dismiss the growth of back-room operations selling authorships to completed scientific manuscripts, predatory journals, coercive citation, or citation cartels as bizarre pathologies, or we may suspect that they reflect market responses to a demand created by the dynamics of a system that is increasingly dysfunctional.

A common response to these changing conditions is to reaffirm our norms and argue that competition and



career pressures do not “justify” misconduct. This is, indeed, as it should be, and for most researchers who abide by the norms that Merton highlighted this is still very much the case. Having said that, we should also recognize that reaffirming norms may be reassuring, but may not address the fundamental institutional and professional factors that are undermining research integrity and our societal reputation. With this in mind, we hope that in the short essay that you agree to write for the symposium issue you consider reflecting on some of the following questions:

- How should our scholarly profession manage the ethics of conflicting issues of entrepreneurialism versus social collaboration and communal ownership?

- How can we manage the advancement of an individual’s “proprietary” scientific views in our highly competitive environment, while simultaneously encouraging collaboration with contemporary and previous scholarship?
- How do we balance the ethics of self-promotion and personal economic returns against the demands of science arguing for communitarianism?
- Finally, and most urgently, how do we maintain a set of cultural values and mores governing our scientific activities against the competing pressures of prestige, competition, and entrepreneurial scholarship?

We very much look forward to hearing from you.

Article Editors: Benson Honig and Joseph Lampel